

THE OHIO
TRANSITIONS TO
INDEPENDENCE
DEMONSTRATION

FINAL IMPACTS
FOR JOBS AND
WORK CHOICE

David J. Fein
Erik Beecroft
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Abt Associates Inc.



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Submitted to:

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EXECUTIVE SUMMARY

As in the current debate over the direction of national welfare reform, much of the inspiration for the 1988 Family Support Act (FSA) -- the last major reform -- was provided by a series of state demonstrations operating under federal waiver authority. Ohio's Transitions to Independence Demonstration was one of the most comprehensive of the pre-1988 FSA waiver initiatives authorized by the Reagan Administration's Low Income Advisory Board.

Transitions to Independence consisted of three separate interventions, each directed to the needs of a different subpopulation of recipients of Aid to Dependent Children (ADC), as Aid to Families with Dependent Children (AFDC) is known in Ohio. The first intervention was a mandatory employment and training program for able-bodied clients without child care responsibilities. Although Fair Work's conception preceded the national Job Opportunities and Basic Skills Training (JOBS) Program created by the 1988 FSA, it resembled JOBS closely. With only minor adjustments to Fair Work, Ohio became, in July 1989, one of the first states to implement JOBS.

The State developed a second approach for single parents with children under age six: a program called Work Choice. As its name suggests, this program was designed to encourage employment through incentives and voluntary employment and training participation. Work Choice's only mandatory element was that clients participate in an employability assessment and counselling session. The principle work incentives were the provision of up to 12 months of transitional Medicaid coverage and child care payments to clients leaving welfare for work.¹

A third demonstration component was focused on teen parents. Project LEARN -- later renamed Learning, Earning and Parenting, or LEAP -- was based on the premise that the most important need for teen parents was to finish high school. It used a combination of financial bonuses and sanctions to promote school attendance.

As a condition for granting federal waivers, Ohio was required to evaluate the implementation, impacts on clients, and cost-effectiveness of each of the three components of the Transitions to Independence Demonstration. The demonstrations were administered by the

¹ At the time Work Choice was designed, the national AFDC program provided only four months of transitional Medicaid and no transitional child care.

Ohio Department of Human Services (ODHS), the State's IV-A agency. This report presents final evaluation findings on the impacts of JOBS and Work Choice on welfare clients' employment, earnings, ADC eligibility, and ADC payments.²

The impact evaluation was based on a classical experimental design, where clients eligible for each program were assigned randomly either to a treatment group that became subject to the new rules and services or to a control group that remained subject to existing rules and services. It is generally accepted that randomly-assigned control groups provide the most rigorous way to assess what would have happened to program participants had they not experienced a change in policies.

Hard evidence on the impacts of Ohio's JOBS program comes at a time of active discussion of JOBS' future in the context of broader welfare reform. Given how widely JOBS program design and implementation have varied across the states and how little is known about the effectiveness of different approaches, findings from each new state evaluation are important.

In many respects, the Ohio findings may provide the best representation of the typical impacts of early state JOBS programs among the few rigorous studies completed so far. Other early state evaluations (e.g., California, Florida) measured impacts for select subsets of mandatory recipients and commenced observation only after the programs had been up and running for a year or more. These research design features, and possibly special attention and assistance provided to demonstration counties in implementing their programs, may help to explain why the JOBS participation rates implied by reported first-year assignment rates in these studies were so much higher than the official JOBS participation rates for these states. Lurie and Hagen's analysis also suggests that Ohio's use of a flexible "choice" model for determining JOBS assignments may have been more typical of early state approaches than the fixed sequence models used in California and Florida. Finally, Ohio's ADC caseload characteristics and benefits closely resemble those of the nation as a whole.

² The State contracted with Abt Associates to evaluate JOBS and Work Choice, and hired the Manpower Demonstration Research Corporation to evaluate LEAP. A series of previous Abt reports describe program implementation (Hollenbeck *et al.* 1990, Lewis *et al.* 1991, and Lewis and Kurth 1992) and early impact and cost-benefit findings (Bell and Fein 1991, Hamilton 1991, Fein *et al.* 1991, and Hamilton 1992). A forthcoming companion report to the present report will present the evaluation's final cost-benefit findings for JOBS and Work Choice.

Although Ohio is of interest because its experience typifies that of early state JOBS programs in many ways, the State also is important for aspects of its program's design and implementation that are distinctive. Of these, perhaps the most noteworthy is the prominence of the Community Work Experience Program (CWEP) in Ohio's JOBS service portfolio. After Nevada, Ohio had the highest percentage of its JOBS participants assigned to CWEP in fiscal year 1992.

Findings from Work Choice are of interest mainly because this demonstration provides one of the few early tests of the Family Support Act's extended transition benefits. Although national attention has moved away from voluntary employment and training approaches, Work Choice's emphasis on transitional Medicaid and child care benefits is quite relevant to current concerns about the need to "make work pay."

In the remainder of this Executive Summary, we describe the basic design of the JOBS and Work Choice demonstrations and review the impact findings. We start with JOBS and conclude with Work Choice.

THE JOBS DEMONSTRATION

ADC clients in 15 counties participated in the Ohio JOBS demonstration.³ All clients who did not qualify for an official exemption from employment and training participation were randomly assigned to treatment and control groups between January 1989 and December 1991 at a ratio of nine to one. Follow-up began in the first month clients became JOBS-mandatory after their county started its JOBS program. In contrast to demonstrations that have waited until some time after start-up to enroll participants, impacts measured here capture the full period of early program experience.

Study subjects were observed until the demonstrations ended in December 1992. Because observation ended at the same time for all clients, those entering the demonstration earlier could be observed for a longer follow-up interval than those entering it later. Clients were grouped

³ The 15 counties were: Brown, Champaign, Clermont, Franklin, Lake, Lawrence, Montgomery, Perry, Pickaway, Richland, Seneca, Stark, Summit, Trumbull, and Wyandot.

for analysis according to their calendar year of demonstration entry. The maximum follow-up possible was three full years for the 1989 cohort; two full years for the 1990 cohort; and one year for the 1991 cohort.

Analyses in this report are based on 4,371 control-group members and a random subsample of 24,120 treatment-group members. Members of the treatment group were subject to all JOBS requirements, including assessment and employment and training participation, whereas members of the control group were neither required nor allowed to receive JOBS services. Both groups were eligible for up to 12 months of transitional Medicaid and child care benefits, as provided by the Family Support Act. The remainder of this section provides the evaluation's main findings.

Ohio implemented a JOBS program which gave caseworkers considerable flexibility in making employment and training assignments.

In contrast to states which moved clients through a prescribed sequence of program activities, Ohio's assessment-driven JOBS program gave caseworkers considerable discretion in assignment decisions. The resulting assignments appear often to have been influenced by client preferences and resource availability. This type of JOBS program design has been referred to in the literature as a "choice model."

County Departments of Human Services (CDHSs) played a large role in implementing and operating Ohio's JOBS program, reflecting the substantial authority and autonomy of county governments in this state. Although caseworkers had considerable discretion in determining individual assignments, general guidelines issued by CDHS's and ODHS nonetheless had a strong influence on placements in the aggregate.

A majority of JOBS-mandatory clients did not receive employment and training assignments. Frequently, clients left ADC, or obtained exemptions or deferrals, before they could be assigned.

As in other states, resources during the early years of Ohio's JOBS program were not sufficient to serve more than a minority of eligible recipients. On average, Ohio reported an

official monthly participation rate of 19 percent for FY 1992, slightly higher than the 16 percent rate for the nation as a whole.

The evaluation's process study found that large backlogs of clients awaiting assessment and assignment persisted through most of the study period. Given the lengthy waits that resulted, many clients left ADC on their own before they could be served. Such delays doubtless go far to explaining the finding that, of all treatment-group members entering the demonstration in 1989, only 40 percent ever received a JOBS assignment during the next three years. Among clients who remained on ADC for at least the first two years of follow-up, the ever-assignment rate (56 percent) is higher than this. Official exemptions, deferrals for cause (e.g., physical or mental health problems), and client refusals to participate also likely account for some nonparticipation.⁴

Over the three years that they could be observed, 4 percent of 1989 cohort members in the treatment group were sanctioned. About two-thirds of the sanctions were for refusal to appear for assessments.

The overall distribution of Ohio JOBS assignments was highly consistent with the Family Support Act's emphasis on basic human capital investments.

Exhibit ES.1 shows that a high proportion of ever-assigned 1989 cohort members received assignments to basic or post-secondary education. Of such clients, 30 percent received basic education assignments and 19 percent received post-secondary program assignments. This emphasis on education -- particularly basic education -- as a route to self-sufficiency in Ohio is consistent with the Family Support Act's stress on human capital investments.

Ohio's early JOBS program assigned a higher fraction of participants to CWEP than any other state save Nevada. Exhibit ES.1 shows that nearly half (47 percent) of all ever-assigned clients were assigned to CWEP. Over time, use of CWEP diminished somewhat, although it remained a prominent assignment.

⁴ The automated systems on which this evaluation relied for participation data did not record deferrals or refusals consistently enough to weigh their contribution to nonparticipation accurately.

Exhibit ES.1

**Patterns of Assignment to Employment and Training Activities
for Early (1989) Demonstration Entrants in the JOBS Treatment Group**

Activity	Percent of <u>All</u> Clients Ever Assigned to Activity ^a	Percent of <u>Ever Assigned</u> Clients Assigned to Activity ^b
Basic Education	12.0	29.6
Post-Secondary Education	7.7	19.0
Job Skills Training	1.6	3.9
Job Readiness Training	4.7	11.6
Job Club	9.0	22.4
CWEP	19.1	47.3
Any Activity	40.5 ^c	100.0 ^c
Number of Clients Observed	7,860	3,184

^a This column shows rates of assignment to each activity calculated using the entire 7,860-member treatment group in the denominator.

^b This column shows rates calculated with the same numerator (number ever assigned to each activity), but with the denominator restricted to the 40.5 percent of treatment group members (3,184 clients) who ever received assignments.

^c Sums for activities do not equal totals for Any Activity because some clients had multiple assignments.

Over the three years of follow-up, JOBS had small positive impacts on employment. However, the program had no statistically significant impacts on total earnings, ADC receipt or ADC payments.

Exhibit ES.2 shows that employment rates for treatment-group members in the 1989 cohort were about three percentage points higher than those for the control group throughout the three year follow-up period. During the third year of follow-up, for example, 45 of treatment-group and 42 percent of control-group members had at least some employment.

The small employment impacts were not accompanied by statistically significant impacts on total earnings, ADC eligibility or ADC payments. Although JOBS did place a somewhat higher fraction of clients in jobs than would have worked otherwise, the additional workers apparently did not work many hours or remain employed long enough to increase earnings or reduce welfare dependency for the mandatory caseload as a whole.

The Ohio impacts are smaller than those found in two other recent JOBS evaluations. Second-year impacts in a study of California's GAIN program were positive across the six demonstration sites for earnings (\$519 and \$370 for clients from single- and two-parent cases, respectively) and negative for ADC payments (-\$347 and -\$469, respectively). First-year results from an evaluation of Florida's Project Independence also show positive earnings impacts (\$157) and negative AFDC payment impacts (-\$157). However, none of these early JOBS evaluations has shown more than modest effects.

Comparison of impacts on total earnings across client subgroups and cohorts suggests that impacts were more favorable for clients with weaker labor force attachment than for those with stronger attachment.

A consistent result in both the JOBS and Work Choice analyses is that impacts tended to be more positive for clients with weaker labor force attachment than for those with greater attachment. Indicators of labor force attachment which support this conclusion include whether clients worked in the six months prior to demonstration entry, whether they were in ongoing or

Exhibit ES.2

JOBS Impacts for Early (1989) Demonstration Entrants^a

Outcome and Follow-Up Interval	Treatment Group (1)	Control Group (2)	Impact [(1)-(2)] (3)	Relative Impact ^b (4)
Percent Ever Employed				
Year 1	40.6	36.9	3.7***	10.0
Year 2	44.2	41.3	2.9**	7.0
Year 3	45.5	42.2	3.3**	7.8
Average Total Earnings				
Year 1	\$1,799	\$1,860	-\$61	-3.3
Year 2	2,639	2,563	76	3.0
Year 3	3,152	3,129	23	0.7
Percent of Months Received ADC				
Year 1	79.9	80.7	-0.8	-1.0
Year 2	60.6	60.5	0.1	0.2
Year 3	48.8	46.7	2.1	4.5
Average Total ADC Payments				
Year 1	\$3,062	\$3,092	-\$30	-1.0
Year 2	2,353	2,366	-13	-0.5
Year 3	1,936	1,845	91	4.9

^a Employment and earnings impacts are based on a total of 1,314 control and 7,860 treatment group members. ADC impacts are based on a total of 1,240 control and 6,807 treatment group cases.

^b The relative impact is the impact expressed as a percentage of the control-group mean: (Column 3/Column 2)*100.

* Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

new/re-opened spells of welfare receipt, and whether they were from single- or two-parent families.⁵

Although not statistically significant, impacts on total earnings were consistently positive for clients with no recent employment, those in ongoing welfare spells, and clients from single-parent families. On the other hand, average earnings were *lower* for treatment- than for control-group members among clients who had been employed in the six months preceding demonstration entry, those just starting welfare spells, and those from two-parent families. The most likely explanation for the latter finding is that some clients worked less than they would have otherwise in order to participate in JOBS activities.

There was much less variability in impacts on ADC payments than on earnings across subgroups. The two subgroups for which opposing, statistically significant payment impacts were found consistently were single- and two-parent families. JOBS led to modest payment reductions among clients from single-parent families but positive payment impacts among those from two-parent families.

Another noteworthy finding was that earnings impacts were positive and statistically significant only among clients with 12 or more years of education: impacts for clients with less education were small and not significant.⁶ Taken as a whole, the subgroup and cohort findings support past research suggesting that mandatory employment and training programs tend to boost earnings more for moderately disadvantaged clients than for the most or least disadvantaged ones. Subgroups for which impacts were most favorable also tended to be those with the highest rates of assignment to post-secondary education programs.

⁵ Starting a new spell of welfare receipt provides a good proxy for labor force attachment because many clients turn to welfare soon after losing jobs. Two-parent families have stronger labor force attachment than single-parent families partly because welfare rules require a recent history of employment as a condition for two-parent ADC eligibility.

⁶ Unlike findings for other subgroups, differences in earnings impacts between these education subgroups were not statistically significant. Similar differences in employment impacts by education were significant, however, providing some basis for trusting the earnings findings.

The absence of more favorable impacts may be attributable to a poor economy and a number of aspects of the implementation and design of Ohio's JOBS program.

Although findings for some subgroups were encouraging, impacts for the entire JOBS-mandatory caseload did not support the hypothesis that employment and training services would generate net increases in welfare clients' self-sufficiency. It is important to consider carefully whether this overall result implies weaknesses in JOBS' basic assumptions, or problems with the program's implementation and/or operating environment. On balance, evidence gathered in the evaluation's process study points strongly to implementation problems in Ohio -- problems similar to those experienced in many other states.

Like most states, Ohio found itself having to provide a wide new array of employment and training services to a broad segment of its ADC caseload with extremely limited resources. Implementation of JOBS coincided with a deep economic recession, which meant that there were fewer jobs for program participants, expanding caseloads of clients needing services, and less tax revenue to support the State's share of JOBS spending.

Among the resulting effects on participants were lengthy waits to assessment and employment and training assignments, low rates of employment and training participation, and insufficient program staff to support many key case management functions. Monitoring clients in JOBS activities and sanctioning for failure to comply with program requirements were among the important case worker responsibilities which were widely neglected.

Ohio's tradition of autonomous county government contributed to the slow pace of JOBS' implementation. Without close State management, counties were left to develop their own approaches to such vital tasks as arranging for employment and training services; prioritizing their client backlogs for JOBS services; and deciding which clients should be assigned to specific employment and training activities.

Program assignments tended to emphasize intensive educational activities with a long-term payoff over those leading to employment more directly. This emphasis, and Ohio's reliance on CWEP, may have limited the potential for positive impacts to emerge within the time period covered by this study. Exploratory analyses conducted in a special sub-study of the evaluation showed that impacts were least favorable for clients who were assigned to CWEP and

basic education, activities which account for a high proportion of JOBS assignments during the demonstration period. In contrast, assignments to job search, job readiness training, and vocational and post-secondary programs were associated with favorable impacts.

These results do not necessarily imply that the clients assigned to basic education would have done better in job search or job readiness training. Abundant evidence in other studies suggests that education is vital to long-term economic independence. At the same time, critics have argued that poor motivation and prior school failure make placement in traditional adult basic education programs a "problematic first step" for many welfare clients. Programs that can ease ADC recipients' concerns about failure and motivate interest in learning deserve urgent attention.

Education and labor-force attachment emerged as distinct dimensions in the impact analysis. Whereas a high school diploma tended to be associated with positive impacts -- a result supported in experimental findings for subgroups as well as by the nonexperimental analyses -- greater work-force attachment led to negative impacts. Clients with greater work-force attachment will more frequently be deferring employment in order to participate in JOBS activities. Whether the deferral is good or bad depends on whether the investments of time pay off in the long-run.

Although we observed clients for up to three years, the slow pace of JOBS' implementation in Ohio and the intensive nature of program assignments imply that long-run impacts could take even longer than this to emerge. Depending on the extent to which future reforms are expected to encounter similar delays, planners may wish to reflect anew on the realism of policies which assume that most clients will be able to achieve self-sufficiency quickly.

THE WORK CHOICE DEMONSTRATION

The second component of Transitions to Independence operated on a demonstration basis only in Montgomery County, a large urban area surrounding Dayton. Work Choice was a voluntary employment and training program directed to single-parent ADC clients whose

youngest child was aged one through five. The demonstration represents one of the few tests of extended transition benefits to date.

Between January 1989 and March 1990, 5,609 clients eligible for Work Choice were randomly assigned to treatment and control groups at a ratio of six to four. Treatment-group members were required to undergo employability assessment and counselling; allowed (and encouraged) to volunteer for the same employment and training services provided to mandatory JOBS clients; and granted eligibility for up to 12 months of transitional Medicaid and child care payments upon leaving ADC for work.⁷ Control-group members were neither assessed nor eligible for program employment and training services, and were eligible only for the pre-Family Support Act transition benefit of 4 months of Medicaid (and no transitional child care).

The original Work Choice demonstration ended earlier than originally envisioned when, starting in April 1990, the State began offering eligibility for twelve months of transitional Medicaid and child care benefits to members of the control group.⁸ Research follow-up continued through December 1990, since control-group members remained ineligible for Work Choice's other components (mandatory assessment/counselling and voluntary employment and training participation) until July 1991 and likely took some time to learn about and respond to extended benefits. The maximum follow-up possible was 18 months for the earliest entry cohort (clients entering between January 1989 and June 1989).

A smaller percentage of eligible clients received employment and training assignments in Work Choice than in JOBS.

By the end of the 18 months over which they were observed, 14 percent of the earliest cohort of Work Choice treatment-group members had volunteered for employment and training activities, about half as many as had received assignments in JOBS at the same follow-up

⁷ Recipients could receive up to \$160 per child each month for child care expenses.

⁸ The extension coincided with the State's general implementation of transition benefits to comply with the 1988 Family Support Act. Although the program ceased to be known as Work Choice, all three Work Choice program components continued as Ohio's program for single parents with young children through June 1991. Starting in July 1991, Ohio added single parents whose youngest child was aged three to five to the mandatory JOBS caseload and discontinued special services for single parents with younger children.

interval. As was true for JOBS, first assignments occurred gradually over the follow-up period. The gradual pace of assignments means that many clients had not been long in activities at the time the demonstration ended.

Work Choice volunteers overwhelmingly chose educational activities.

The vast majority of Work Choice assignments were to education and training activities, of which basic and post-secondary programs accounted for a lion's share. Among early demonstration entrants volunteering for activities, 34 percent received assignments to basic education and 33 percent received assignments to post-secondary programs (See Exhibit ES.3). A smaller fraction (12 percent) of assignments was to job club, and hardly any clients (1 percent) volunteered for CWEP.

Interviews reported in the evaluation's process study suggested that JOBS clients and caseworkers tended to view CWEP as "working off your grant" rather than as an affirmative opportunity for job experience. The Work Choice assignment patterns confirms that CWEP was not a popular program activity.

The predominance of educational assignments in Work Choice suggests that most of the potential positive impacts of this component of Work Choice on self-sufficiency could not be expected to emerge within the follow-up period covered in the evaluation. Some positive impacts might arise sooner among the small group of clients assigned to job club and job readiness, however.

Only four percent of treatment-group members and three percent of control-group members used extended Medicaid sometime during the 18-month follow-up period.

Incentives created by extended transition benefits also might have a positive short-run impact on employment for the most employable clients. Our findings suggest that the size of this effect was small, at best. The percentage of clients ever leaving ADC and receiving transitional Medicaid was only one point greater for the treatment group. Although small, this one-percentage point impact was statistically significant. On the other hand, it is not necessarily

Exhibit ES.3

**Patterns of Assignment to Employment and Training Activities
for Early (January - June 1989)
Demonstration Entrants in the Work Choice Treatment Group**

Activity	Percent of <u>All</u> Clients Ever Assigned to Activity ^a	Percent of <u>Ever</u> <u>Assigned</u> Clients Assigned to Activity ^b
Basic Education	4.8	34.2
Post-Secondary Education	4.7	33.4
Job Skills Training	2.5	17.7
Job Readiness Training	2.3	16.0
Job Club	1.7	11.7
CWEP	0.2	1.4
Any Activity	14.1 ^c	100.0 ^c
Number of Clients Observed	2,601	368

^a This column shows rates of assignments to each activity calculated using the entire 2,601-member treatment group in the denominator.

^b This column shows rates calculated with the same numerator (number ever assigned to each activity), but with the denominator restricted to the 14.1 percent of treatment group members (368 clients) who ever received assignments.

^c Sums for activities do not equal totals for Any Activity because some clients had multiple assignments.

the case that extended Medicaid was responsible for this impact: other Work Choice components -- such as counselling and participation in employment activities -- also could have contributed to increased welfare-to-work transitions.

Work Choice had small positive impacts on net employment and earnings.

Exhibit ES.4 shows that net employment rates were about three percentage points higher for members of the treatment group than for members of the control group over the 18-month follow-up period, and that average total earnings were about \$177 higher. The fact that the net employment impact was greater than the impact on welfare-to-work transitions (two percentage points) suggests that the program also increased the fraction of clients combining welfare and work.

Despite the positive earnings impacts, Work Choice did not reduce ADC payments. This finding suggests that the total amount of the earnings gain was not sufficient to offset amounts disregarded in grant calculations for work expense, child care and work incentive allowances.

The largest positive impacts on total earnings were found for clients with the weakest labor-force attachment and those with 12 or more years of education.

Variation in Work Choice impacts by subgroup and demonstration cohort bears a strong similarity to findings from the corresponding analyses for JOBS. Although differences in Work Choice impacts across subgroups were not statistically significant, the direction, magnitude and individual significance levels for point estimates strongly support this conclusion.

For example, Work Choice had statistically significant positive impacts on average total earnings for clients who had not worked in the six months preceding demonstration entry and for clients with 12 or more years of education. Impacts for clients who had recent work experience and those with less education tended to be negative, or slightly positive, and not significantly different from zero.

As in the case of JOBS, impacts on average total earnings were negative for later (July to December 1989) Work Choice entrants, a cohort containing mostly clients just starting welfare

Exhibit ES.4

**Work Choice Impacts for Early (January - June 1989)
Demonstration Entrants^a**

Outcome and Follow-Up Interval	Treatment Group (1)	Control Group (2)	Impact [(1)-(2)] (3)	Relative Impact ^b (4)
Percent Ever Employed				
Year 1	35.2	31.8	3.4**	10.7
Year 2 (First 6 months)	28.2	24.9	3.3**	13.3
Average Total Earnings				
Year 1	\$900	\$791	\$109*	13.8
Year 2 (First 6 months)	584	516	68	13.2
Percent of Months Received ADC				
Year 1	86.8	86.5	0.3	0.3
Year 2 (First 6 months)	76.5	75.9	0.6	0.8
Average Total ADC Payments				
Year 1	\$3,389	\$3,390	-\$1	-0.03
Year 2 (First 6 months)	1,564	1,553	11	0.7

^a Impacts are based on a total of 1,619 control and 2,536 treatment group members.

^b The relative impact is the impact expressed as a percentage of the control-group mean:
(Column 3/Column 2)*100.

* Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

spells. Work deferment among clients with the greatest labor force attachment seems a plausible explanation for negative impacts for voluntary, as well as mandatory, employment and training participants.

The limited follow-up possible in the Work Choice demonstration prevents a definitive conclusion regarding the merits of this approach.

In the short run, Work Choice generated modest increases in both employment and total earnings among early demonstration entrants. The research design does not allow us to determine exactly which of the program's three main elements -- voluntary employment and training participation, mandatory assessment/counseling, or extended transition benefits -- was responsible for the observed impacts. However, at best, none of these factors could have had a very large effect.

It is not clear that we should expect to see strong positive effects over the short time horizon that was observable in this demonstration. Clients eligible for Work Choice had relatively low employability at the outset, suggesting that few may have been in a position to take immediate advantage of transition benefits or employment activities. The potential for positive impacts in the long term rests with the success of those clients who took advantage of the opportunity to volunteer for education and training provided by Work Choice.

CONCLUSIONS

Six years after launching the Transitions to Independence Demonstration, Ohio now stands at the brink of a new round of welfare reform. Three new initiatives -- Communities of Opportunity, Families of Opportunity, and Children of Opportunity -- go substantially beyond the employment and training strategy. The new policies test new incentives to work; expansions in supportive services; incentives to family stability; broadening of the ages of children subject to learnfare; employment subsidies; and economic development.

The latest Ohio reforms embody a growing national sense that services created by the Family Support Act are not enough to substantially end the need for welfare. This sense appears

to be grounded both in the direct observation that welfare caseloads do not appear to have been much affected by JOBS and related services during the program's early years, and by early evaluations finding only modest impacts.

Many observers argue that the program's early years must not be judged too harshly. During these years, state and federal funding for the Family Support Act never approached the levels required for full implementation. Clients emerging from JOBS employment and training activities in the midst of a deep recession also faced an unusually difficult job market during this era.

Still, the findings presented in this report tend to reinforce the general consensus that, by itself, the employment and training strategy -- whether mandatory or voluntary -- so far has produced quite modest results. Neither JOBS nor Work Choice produced statistically significant overall impacts on ADC participation or payments. Although the two programs did have impacts on employment and earnings, these effects were quite modest and not always positive in direction.

Consideration of the Ohio evidence does not necessarily lead to the conclusion that JOBS and Work Choice were based on faulty premises about what welfare recipients need to become self-sufficient. One alternative hypothesis is that limitations in program implementation, rather than deficient program design, limited the magnitude of the impacts. Another possibility is that employment and training and supportive services are *necessary* components of an effective approach but not *sufficient* in themselves to help a substantial portion of recipients leave welfare. Even under the best economic conditions, it is hard for welfare recipients to find jobs whose compensation compares favorably with welfare benefits (including Medicaid and food stamps). The recession which prevailed through most of the demonstration years (1989-92) both limited job availability and required spreading JOBS resources more thinly across an expanded caseload.

Both interpretations offer reasons to be hopeful that JOBS and related services could lead to stronger positive impacts in Ohio in the future. Even during the difficult years in which Ohio's JOBS and Work Choice programs were implemented, both programs were able to help many clients to improve their skills and earnings. Indeed, to the extent that the State has moved past early implementation problems and out of the recession that characterized the years covered by this study, its continuing JOBS program already may be generating more favorable outcomes.

CHAPTER ONE

INTRODUCTION

In 1988, Ohio received federal waivers to operate the Transitions to Independence Demonstration, a reform of the State's Aid to Dependent Children (ADC) program anticipating a number of important elements of national welfare reform policy. The demonstration tested three interventions, each targeted to a different segment of the ADC caseload. The first, Fair Work, was a mandatory employment and training program for adult recipients whose youngest child was age six or older. The second, Work Choice, was a voluntary program for single parents whose youngest child was between ages one and six. The third, Project Learn (later renamed Learning, Earning and Parenting, or LEAP), was a program aimed at bolstering school attendance among teenage parents.

The terms and conditions of the waiver required all three programs to be rigorously evaluated using a random-assignment design. Abt Associates Inc. was contracted to evaluate Fair Work and Work Choice.¹ Previous reports under this contract have described the implementation and early impacts of these programs.² This report presents our final estimates of the impacts of Fair Work and Work Choice on clients' employment, earnings, ADC participation, and ADC benefits.

The Fair Work component of the evaluation provides one of the earliest tests of the national Job Opportunities and Basic Skills Training (JOBS) Program. Although its design preceded the 1988 Family Support Act (FSA), Fair Work closely resembled JOBS. When Ohio formally converted Fair Work to JOBS in July 1989, it became one of the first states to implement the national program. The decision to continue the demonstration for another three years puts Ohio in the position of having one of the first JOBS programs to be rigorously

¹ The third component is being evaluated separately by the Manpower Demonstration Research Corporation. Project Learn -- subsequently renamed the Learning, Earning and Parenting (LEAP) Program -- uses bonuses and sanctions to increase school attendance among teenaged parents. See Bloom et al. (1993) for further information.

² Implementation of Ohio's JOBS program is described in reports by Hollenbeck et al. (1990), Lewis et al. (1991), and Lewis and Kurth (1992). Early program impacts and cost-benefit findings are summarized in Bell and Fein (1991), Hamilton (1991), Fein et al. (1991), and Hamilton (1992). Patterns and effects of assignments to different JOBS employment and training activities are summarized in Fein (forthcoming). Lewis (1993) discusses participants views of their JOBS experiences and the program's effects on non-pecuniary outcomes.

evaluated. Each early JOBS evaluation is especially significant in light of the great variation in state and local program designs fostered by national legislation.

The Family Support Act requires states to offer welfare recipients a wide array of services aimed at increasing self-sufficiency, but leaves states free to tailor these services to their own environments. At the Act's heart is the JOBS program, which requires non-exempt clients to have their employability assessed and to participate in employment and training activities. Federal JOBS rules specify some activities that all programs must offer, but leave the choice of others up to states.³ States also have considerable flexibility in determining the overall level of client participation and rules for assigning clients to different employment and training activities. As a consequence, there is considerable variability across states in the extent to which program participation is enforced and in the relative emphasis on human capital investment and immediate employment strategies. Federal flexibility also has allowed states and localities to draw on a wide range of administrative mechanisms, funding sources, and service providers in implementing JOBS (Lurie and Hagen 1993).

Given wide variation in program design and operations, it is important to identify the key distinguishing features of each JOBS program being evaluated. Much of the present chapter therefore will be devoted to describing the salient features of Ohio's JOBS program and environment.

Ohio's JOBS program serves the nation's third largest ADC caseload. The State IV-A agency responsible for administering JOBS -- the Ohio Department of Human Services (ODHS) -- provides overall coordination, policy direction, and monitoring, but, as in many large states, County Departments of Human Services (CDHS's) have substantial autonomy in program design and operation. As a result, the structure and operations of the program vary considerably across counties. Nonetheless, procedures for determining assignments to employment and training activities in different Ohio counties do follow a common underlying logic.

³ States must offer education activities, job skills training, job readiness activities, and job development and placement services. States also must offer at least two of the following four activities: job search, on-the-job training, work supplementation programs, and community work experience programs.

Ohio's JOBS program model may be described as a fusion of the "choice" and "educational screening" models described by Lurie and Hagen (1993). The State's emphasis on up-front assessment provides great latitude to caseworkers in making assignments and also accords a strong role to client preferences.⁴

As required by national JOBS rules, the State encourages CDHSs to assign clients without a high school diploma or equivalent to a basic education program. Although many basic education assignments are made, a significant fraction of clients opt not to receive these assignments. For them, assignment to a Community Work Experience Program (CWEP) site is a frequent recourse. Clients with a high school diploma or GED have many more options. Depending on individual circumstances and available resources, such clients may be assigned to post-secondary programs, job readiness activities, skills training, job search, or CWEP. CWEP, an unusually important component in Ohio, frequently is used as an assignment of "last resort." In 1992, one out of every five Ohio JOBS participants (excluding those participating in assessments) was a CWEP assignee -- more than any other state except Nevada.⁵ Ohio's program also assigns a larger share of clients to post-secondary programs than many other states.⁶

Many factors other than the basic design of JOBS programs can influence their effectiveness. Even the best-designed program will be ineffective if it is not well implemented. Local labor markets must be able to generate a sufficient number of jobs. Finally, results as measured by evaluations may vary solely because of the way in which the evaluations are designed. These issues will be treated at some length in the remainder of this chapter. In the following three sections we summarize the origins of the Transitions to Independence

⁴ The other two states whose JOBS programs have been rigorously studied -- California and Florida -- use more formal tracking systems and rely less on intensive preliminary employability assessment. After a simple initial determination of need for basic education, most clients in California's GAIN program are assigned to either basic education or job search; only after these initial activities are completed are they formally assessed. Florida's Project Independence screens clients on job readiness. Those determined to be job ready are assigned to job search; only the remainder are formally assessed and then referred to appropriate education and training activities.

⁵ In Nevada, 22 percent of participants were in CWEP; nationwide, only 4 percent received this assignment (U.S. Department of Health and Human Services 1994).

⁶ Differences in proportions of JOBS participants assigned to post-secondary activities (including self-initiated activities) are especially marked between Ohio (23 percent), California (5 percent), and Florida (9 percent). Nationwide, 18 percent of JOBS participants were in post-secondary activities.

Demonstration and describe Ohio's JOBS and Work Choice programs in more detail. Next, we summarize the research design that was used to measure program impacts. The chapter concludes with an assessment of the social and economic environments in which Ohio's JOBS programs operated during the years spanned by the demonstration.

1.1 WELFARE REFORM IN OHIO BEFORE JOBS

Ohio first operated mandatory work programs for ADC recipients under WIN demonstration authority provided in the Omnibus Budget Reconciliation Act of 1981. The Ohio Work Programs mainly emphasized CWEP assignments, but also included job search, education and training, and subsidized employment components. The program reportedly achieved high participation levels, especially among two-parent cases (Schiller and Brasher 1993).

The State revamped its mandatory ADC work program in 1988, under a waiver granted by the White House Low Income Advisory Board. The resulting program -- Fair Work -- was very similar to JOBS. Fair Work required clients to be assessed and to participate in an employment and training activity. Compared with Ohio Work Programs, Fair Work placed greater emphasis on education and training. Exemptions from participation were granted under circumstances similar to those in JOBS.⁷

By 1988, when Fair Work was implemented, 29 of Ohio's 88 counties were operating Ohio Work Programs for all or a portion of their caseloads: these were the first to convert to Fair Work. In January 1989, 13 additional counties implemented Fair Work. The State also initiated the other two components of Transitions to Independence -- Work Choice and Project Learn (LEAP) -- at this time.

Planning for Transitions to Independence already was well under way in 1988, when Congress passed the Family Support Act. Ohio was able to modify Fair Work in short order and, in July 1989, became one of the first states to implement JOBS.

⁷ The Fair Work child care exemption applied to parents with children under age six, whereas the JOBS exemption applied only to parents with children under age three. Fair Work generally exempted one parent in two-parent cases, whereas under JOBS both parents are mandatory unless they have some other exemption.

Although the State decided to continue Project Learn, Work Choice ceased to be known by that name after March 1990, when -- in accordance with the FSA -- the State began offering JOBS transitional child care and Medicaid to all clients terminating ADC due to employment; including control-group members. However, the mandatory assessment - voluntary education and training elements of the original Work Choice experiment continued in place until June 1991.

Implementation of JOBS and Work Choice was a gradual process. Counties initially faced huge backlogs of existing cases and only gradually expanded their staffing, funding, and access to services. As documented in prior evaluation reports, the State's early JOBS years were characterized by lengthy waits to assessment and assignment, and inconsistent monitoring of assignees (Hollenbeck *et al.* 1990; Lewis *et al.* 1991; and Lewis and Kurth 1992).

1.2 ADMINISTRATION AND DESIGN OF OHIO'S JOBS PROGRAM

Overall responsibility for policy, coordination, and monitoring of Ohio's JOBS program rests with the Ohio Department of Human Services (ODHS). Early on, ODHS prioritized developing financial agreements with other state and local agencies providing services to JOBS clients that would help the State to maximize its ability to match Federal JOBS funds. Compared with most other states, Ohio has been quite successful in drawing down its Federal allotments. In fiscal years 1990, 1991, and 1992, the State captured 49 percent, 76 percent, and 85 percent of its maximum Federal allocation:⁸ total JOBS spending in Ohio (federal, state and local) for these years was \$40 million, \$76 million, and \$86 million, respectively.

Much responsibility for managing JOBS in Ohio is vested with the state's 88 County Departments of Human Services (CDHS's). County autonomy has resulted in a wide range of organizational arrangements. A 1990 survey of 39 of the 42 counties then operating JOBS found that 17 programs were located in income maintenance units, 15 were in county social service units, and 7 operated as independent units (James Bell Associates, Inc. 1990).

⁸ The national figures for these years are 33 percent, 56 percent, and 68 percent, respectively. Ohio and U.S. figures for FY 1990 are low in part because not all localities and states had implemented JOBS.

The approach in Ohio corresponds to the "welfare agency" model for organizing JOBS services identified by Lurie and Hagen (1993). In this model, the "state- or local-level welfare agency maintain[s] full responsibility for operating the program, taking the lead in servicing recipients in-house or in contracting with other agencies for selected services," in contrast to an "integrated system" approach, where JOBS administration is largely subsumed within the broader state employment and training system.

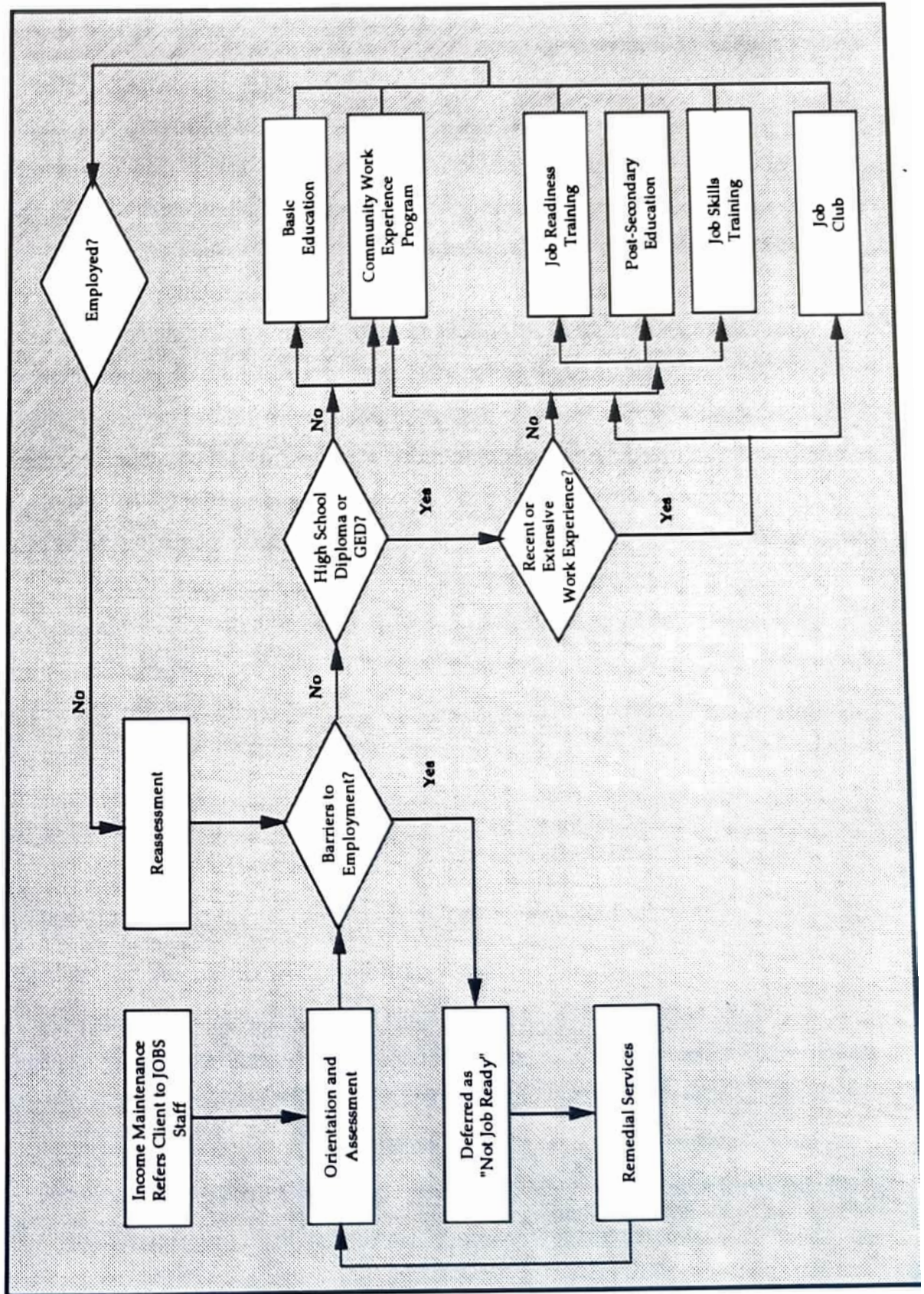
CDHS's provided some JOBS services themselves and typically signed contracts and other agreements with a wide range of public and private organizations for others. At one extreme, Lake County's CDHS contracted with the local JTPA agency -- the Lake County Employment and Training Administration -- to directly provide or arrange most of its essential JOBS services. Lake was the only county to vest so much operational responsibility in an outside agency, however. More typically, CDHS's offered a series of basic services (e.g., assessment, employability planning, job club, and CWEP placement) in-house and procured other services from providers such as JTPA agencies, the Ohio Bureau of Employment Services (OBES), local transportation authorities, public schools and community colleges, community action agencies, Goodwill Industries, and private firms.

Although the operations of local JOBS programs in Ohio vary considerably, it is nonetheless possible to identify a distinctive underlying approach to determining employment and training assignments. Exhibit 1.1 summarizes the Ohio program design.

Defining features of this model are the discretion it gives to caseworkers and its emphasis on educational screening. The process begins when CDHS income maintenance workers refer JOBS-mandatory clients to the JOBS program unit. JOBS staff notify these clients to come in for orientation and assessment. Assessment interviewers concentrate, first, on identifying "barriers" to employment, such as health problems or the unavailability of transportation or child care needed to work or participate in JOBS. Clients with such barriers are excused from participation and may be referred to remedial services (e.g., substance abuse treatment).

Interviewers then ascertain clients' education, work histories and occupational interests. This information becomes the basis for client "employability plans," which describe clients' occupational goals and identify the activities to be pursued to reach them. The specifics of each

Exhibit 1.1 Simplified Depiction of Ohio JOBS Program Design



plan are arrived at through varying contributions of caseworker discretion, client preferences, and the availability of services.

In accordance with State guidelines, clients without a high school diploma or equivalent normally are encouraged to enroll in basic education activities such as Adult Basic Education, English as a Second Language, and GED preparation. The guidelines allow clients to opt out of basic education, and many appear to do so.⁹ The most typical recourse for such clients is assignment to a Community Work Experience Program (CWEP) site.

A legacy of the Ohio Work Programs, CWEP is an important part of Ohio's JOBS program. Clients receiving this assignment are placed with a public or nonprofit agency to perform public service in exchange for their ADC grants. Although the ostensible purpose of CWEP is to provide work experience, in Ohio it frequently is used as an assignment of "last resort:" either as a primary assignment for clients who have not succeeded at, or do not wish to be placed in, other activities; or as a secondary assignment for clients whose primary assignments require less than 20 hours per week. In 1990, the average CDHS had placement arrangements with 51 different CWEP sites (James Bell Associates, Inc. 1990).

Clients with a high school degree or equivalent could be placed in a far wider range of possible assignments. Those with little work experience might be assigned to CWEP, or to one of the following education and training activities:

Post-secondary education. Ohio assigns a relatively high percentage of JOBS participants to post-secondary training programs. Through the Ohio Board of Regents, JOBS provides these clients with special school retention services.

Job readiness training (also called employability development services). These services, which aim to train participants in the attitudes and behaviors needed to succeed in the workplace, were offered by the CDHS's, JTPA, OBES, and local colleges.

Job skills training. Courses are offered by vocational schools, post-secondary institutions and other organizations. Common courses teach cosmetology and barbering, truck driving, secretarial skills, and computer operation.

⁹ HHS data for FY 1992 indicate that whereas 49 percent of JOBS participants statewide had not completed 12 years of school or received a GED, only 36 percent of participants were in basic education programs.

Clients with recent work experience and a high school degree ordinarily would be assigned to job club, Ohio's job search activity. Job club typically consists of three weeks of group training in goal setting, resume preparation, and job search and interviewing techniques; followed by a period of individual job search. As indicated in Exhibit 1.1, however, job-ready clients also could be assigned to an education or training activity, or -- as a last resort -- to CWEP. State rules allow clients to bypass job club when caseworkers determine that clients are either sufficiently familiar with job search techniques or have participated in job club for at least 60 days during the past 12 months.

In sum, Ohio's JOBS program uses an assessment-driven approach which gives caseworkers substantial discretion in making placements. The assignment portfolio is oriented heavily towards education and training activities, and is noteworthy for its intensive use of post-secondary education and CWEP.

1.3 THE WORK CHOICE PROGRAM

The second component of Transitions to Independence, Work Choice, operated in Montgomery County between January 1989 and March 1990. This program targeted a segment of the caseload not originally covered by Ohio's JOBS Program--clients whose youngest child was between the ages of one and five. The first two elements of Work Choice were mandatory: an assessment of job readiness and vocational counseling. Except when subject to the categorical exemptions for Fair Work/JOBS or when assigned to the control group, all ADC-R case heads with children between the ages of one and five were subject to these requirements. Those who failed to appear for assessment without good cause could be sanctioned with a loss of benefits for up to two months.

Following assessment and counselling, participants were told they could receive all of the employment-related services offered by JOBS, should they choose to volunteer. Participation in these activities was not required, however, and the other program benefits described below were made available regardless of participation in employment and training activities.

During assessment, participants were offered child care and extended Medicaid coverage if they should leave ADC due to earnings. The Medicaid extension was designed to ease the

transition from welfare to private employment by compensating for the lack of health insurance in many entry-level jobs. Under previous rules, clients who lost their ADC eligibility because of employment could continue their Medicaid eligibility for only four months. Work Choice participants who left ADC due to employment were eligible instead for up to twelve months of post-ADC Medicaid coverage. The same individuals qualified for reimbursement of child care expenses through Work Choice, up to \$160 per child per month for twelve months. No post-ADC child care benefits were provided under previous ADC rules.

In encouraging single mothers with young children to participate in employment and training on a voluntary basis, Work Choice reflected the view that requiring poor single mothers to work could have negative consequences on the quality of care and development of young children. Although interest in voluntary employment and training programs has waned in recent years, the demonstration's two other key elements -- expanded post-ADC child care and health insurance -- are alive and well in the current policy environment. The FSA extended 12 months of transitional child care and Medicaid benefits to all families leaving welfare for work, and current reform proposals would go even further. Although abundant anecdotal evidence supports such expansion, there still has been no rigorous evaluation of the effectiveness of the transition benefits provided by the FSA. Albeit in a voluntary context, Work Choice remains the only demonstration to test transition benefits to date.

1.4 RESEARCH DESIGN

Both the Ohio JOBS and Work Choice demonstrations used experimental designs to measure program impacts on self-sufficiency. Clients subject to each of the programs were randomly assigned to either a treatment group, which was subject to the new program, or to a control group, which continued under old rules. The control group's experiences represent the "counterfactual" for clients assigned to the treatment: that is, the experiences treatment-group members would have had if they had not been exposed to the intervention. Outcomes are measured at regular intervals after sample entry, and the difference in mean outcome levels for treatment- and control-group members is taken as a measure of program impact. In this section,

we describe the general research design and then identify distinctive aspects of the JOBS and Work Choice demonstrations.

Timing of Random Assignment and Sample Enrollment

In both demonstrations, random assignment (and sample entry) occurred when clients first were determined to meet the criteria for program eligibility. Observation commenced at this time, regardless of when those assigned to the treatment group actually first received program services. Impacts measured thus capture program effects on all clients who were subject to the program.

This design may be contrasted with approaches in which random assignment and sample entry come later in the process of enrolling clients in the program. A notable example of the latter is the California GAIN demonstration, where only clients who showed up for program orientation were randomly assigned and evaluated.¹⁰ A shortcoming of this design is that impacts measured do not capture any impacts for no-shows or any pre-orientation impacts for clients who do show.¹¹ A compensating strength is that impacts measured provide a better approximation to the effectiveness of a fully-implemented program -- one where a high fraction of the caseload is served.

Another important aspect of the timing of sample enrollment in Ohio is that nearly all counties were just implementing the new program when random assignment began. This design may be contrasted with demonstrations where sample enrollment is delayed to give programs a chance to come closer to their operational "steady states." The California and Florida JOBS evaluations are of the latter type.¹²

¹⁰ In another JOBS evaluation -- of Florida's Project Independence -- sample entry coincided with the determination of mandatory JOBS status, as in Ohio.

¹¹ In principle, mandatory programs such as JOBS are more likely to have such effects than voluntary programs (Gueron and Pauly 1991). Examples include clients who, learning of their new obligations, quit ADC for work (becoming no-shows) and those who respond in an opposite fashion, reducing work hours to take advantage of JOBS's new education and training opportunities.

¹² These evaluations both delayed sample entry for about a year after program start-up to allow programs to come closer to their operational "steady states." In Florida, this design imposed a substantial cost on the generalizability of study findings, since it meant that only new clients (who hadn't been previously exposed to JOBS)

It can be expected that impacts measured during a period of program implementation will be more modest than those measured thereafter. This point should be born in mind when comparing results for the earliest cohort of Ohio demonstration enrollees (those entering the study in 1989) with results for demonstrations where random assignment commenced some time after program start-up.

Conceptually, each design has advantages and disadvantages. Waiting a year to begin random assignment allows a program to move past the most difficult early phase of implementation, but by no means guarantees that the impacts measured will describe a "mature" program. Lurie and Hagen's (1993) study of how ten states implemented JOBS suggests that implementation in most states has taken years, rather than months, and is in many respects still incomplete. Although the Ohio design may tend to understate the effectiveness of programs that are up and running, there is less ambiguity concerning the portion of implementation which is being described.

Measuring Program Impacts

Impacts are estimated by comparing outcomes for average members of the treatment and control groups in successive time periods following sample entry. The principal outcomes measured in this study are employment, earnings, ADC eligibility, and ADC payments.

Employment and earnings outcomes are measured using quarterly wage data reported by employers to Ohio's Bureau of Employment Services (OBES), as required by the federal Unemployment Insurance (UI) program. As measured here, employment is defined as the presence of any reported wages during a specified time interval. ADC eligibility and payment measures are derived from information on regular monthly extract files taken from the State's automated Client Registration Information System (CRIS) and its successor, CRIS-E[nhanced].

During random assignment, chance differences sometimes arise in the characteristics of clients assigned to the treatment and control groups. All impact estimates presented in this report have been "regression-adjusted" to reduce the influence of such differences. The

could be enrolled in the demonstration.

procedure essentially consists of regressing each outcome variable on a dummy variable representing treatment-control assignment (1 = treatment, 0 = control) and a series of other client characteristics measured as of the point of sample entry.¹³ Extracts from CRIS, CRIS-E, and the UI databases also provided the data used to measure these characteristics. More details on the variables included in impact models are provided in Chapters 3 and 4.

In addition to impacts for the entire sample, this report also presents impacts for important client subgroups. Subgroups were chosen for the analysis on the basis of their social or policy significance and the expectation that they might respond to the intervention in a distinctive manner. Subgroup impacts were derived by estimating separate regression impact models for each subgroup.

All clients moving out of the demonstration area who received ADC in a non-demonstration county were removed from observation from the month of migration on. Their removal was necessary because non-demonstration counties did not necessarily withhold services from control-group members.¹⁴ An effect of excluding out-migrants is that sample sizes decline slightly over successive intervals of follow-up.

JOBS Demonstration

Fifteen counties implemented JOBS on a demonstration basis on or soon after January 1989.¹⁵ Random assignment of newly JOBS-mandatory clients continued through December 1991. In all but two counties, the entire mandatory caseload eventually underwent random

¹³ Ordinary least squares regression was used to estimate impacts for all outcomes except for employment, for which logistic regression models were employed.

¹⁴ We were not able to identify clients moving to non-demonstration counties in Ohio who did not receive ADC or those moving out of state. This type of migration is not expected to have much effect on the integrity of the control group, however: control-group members moving in-state and not receiving ADC were not exposed to JOBS, and those moving out-of-state likely constitute a relatively small fraction of total movers.

¹⁵ The 15 were: Brown, Champaign, Clermont, Franklin, Lake, Lawrence, Montgomery, Perry, Pickaway, Richland, Seneca, Stark, Summit, Trumbull, and Wyandot.

assignment.¹⁶ A third county -- Franklin -- first implemented the demonstration for only two-parent (ADC-U) cases, and did not begin enrolling ADC-R's until January 1991.

Sample entry occurred in the month that income maintenance workers first determined that a client was required to participate in JOBS. Overall, 54 percent of adult ADC recipients in Ohio were JOBS-mandatory in FY 1992, a higher fraction than adult recipients nationwide (43 percent).¹⁷ IM workers generally referred all mandatory clients -- treatment and control -- to the JOBS units in their county; JOBS staff subsequently only provided services to members of the treatment group.¹⁸

Between 1989 and 1991, the entire caseload in the 15 demonstration counties was randomly assigned to treatment and control groups at the ratio of nine to one. Analyses in this report are based on 4,371 control-group members and a random sub-sample of 24,120 treatment-group members.¹⁹

As noted above, random assignment of new cases ended on December 31, 1991. The demonstration continued another full year for previously-assigned cases however. Because enrollment stretched over three years, the maximum available follow-up varies by cohort.

¹⁶ Montgomery and Stark counties began operating work programs for some mandatory clients before 1989. In Montgomery, the work program served a broad segment of the mandatory caseload, and random assignment was restricted to cases opening after December 1988 that had not been previously active and mandatory in the county. In its pre-demonstration work program, Stark served a narrower segment of its mandatory caseload (two-parent cases and cases without children under age 15), but subsequently experienced serious operational difficulties in distinguishing demonstration participants; accordingly, only clients first becoming JOBS-mandatory after December 1990 are included in the research sample.

¹⁷ U.S. Department of Health and Human Services (1993) statistics for FY 1992. The percentage mandatory in Ohio prior to June 1991 was much lower because the State still was exempting clients for care of children aged three to five.

¹⁸ Random assignment was implemented on the State's automated systems, which employed an algorithm based on client social security numbers. Treatment-control status information for each client was indicated on the CRIS and CRIS-E systems and on referral forms received by JOBS offices. In addition, ODHS sent each county a monthly roster listing all control-group members. Varying county intake procedures and delays between referral and receipt of the rosters caused confusion for some assignment statuses, resulting in the provision of JOBS services to 12 percent of control-group members. These clients were identified and removed from the sample on which this analysis was based. Multivariate regression techniques were used to weight the remaining control-group members so that their composition closely mirrored that of the original control group population.

¹⁹ Although initial treatment group sub-sampling occurred at the person level, all JOBS mandatory clients in sampled persons' cases were included in the final analysis sample. As two-parent (ADC-U) cases often had two persons eligible for inclusion at initial sampling, these cases were disproportionately likely to be selected. Accordingly, a second set of weights was used to ensure that the ADC-R/ADC-U ratio matched the ratio of cases in the treatment and control group populations.

Clients entering the sample in 1989 could be observed for three full years, those entering in 1990 could be observed for two years, and those entering in 1991 could be observed for only one year.²⁰

Work Choice Demonstration

Clients subject to Montgomery County's Work Choice demonstration were heads of single-parent (ADC-R) cases with a youngest child aged 1-5 who were not otherwise exempt according to JOBS rules. Statewide, such clients represented about two-fifths of all cases. A total of 5,609 clients was enrolled in the demonstration between January 1989 and March 1990. These clients were randomly assigned to the treatment and control groups at the ratio of six to four.

As previously mentioned, the Work Choice demonstration changed substantially after March 1990, when the State extended eligibility for transitional child care and Medicaid benefits to control-group members leaving ADC due to employment. Two other important components of the Work Choice "treatment" remained in place on an experimental basis through June 1991: mandatory employability assessment and the option of volunteering for employment and training services.

It likely took some time after March 1990 for members of the Work Choice control group to learn of their eligibility for extended transition benefits, and additional time for them to act on this knowledge. Accordingly, follow-up in the analyses presented in this report is extended through December 1990.

Clients enrolled in Work Choice early in 1989 were exposed to the full treatment longer than those enrolled later in the demonstration. To distinguish those with more and less exposure to the transition benefits component of the demonstration, separate impacts are estimated for clients who were enrolled during the demonstration's first six and last six months.

²⁰ These follow-up intervals represent maximums for each cohort of demonstration entrants as a group. As a rule, cohort members entering earlier in a given year could be observed somewhat longer than the cohort as a whole.

1.5 DEMONSTRATION ENVIRONMENT

The social and economic environment in which JOBS operates can profoundly influence the program's capacity to foster self-sufficiency. Aspects of the social environment that foster a culture of long-term welfare dependency could make it harder for JOBS to motivate ADC clients to pursue self-sufficiency. Local economies also may vary in their capacity to provide jobs to low-skill workers.

This section reviews several aspects of the JOBS environment in Ohio. First, we describe some basic characteristics of the fifteen demonstration counties and compare these characteristics with those of Ohio and the United States. Next, we discuss ADC caseload trends in Ohio and the U.S. and discuss some of the factors besides JOBS which may account for these trends.

Social and Economic Characteristics of the Demonstration Counties

As shown in Exhibit 1.2, the 15 JOBS demonstration counties are located around Ohio's southwest to northeast axis. Although pre-existing work programs in 29 counties precluded selecting a random sample of counties for the JOBS demonstration, the 15 counties chosen do reflect a diverse set of social and economic conditions.

Exhibit 1.3 shows that demonstration counties included several of Ohio's largest urban counties (e.g., Franklin, Montgomery, Summit); a number of mid-sized suburban counties (e.g., Clermont, Lake); and a handful of smaller, rural counties (e.g., Brown, Champaign, Wyandot). Together, the 15 counties accounted for 32 percent of Ohio's 1990 population and 30 percent of the State's 1989 ADC caseload.

The economies of the demonstration counties were quite varied. Typically, employment in service industries was greatest in the large urban counties, whereas manufacturing jobs accounted for a higher proportion of workers in small and mid-sized counties. Although agriculture is an important sector of the economy in a number of the rural demonstration counties, this sector accounted directly for only a small fraction of workers.

Exhibit 1.2 Counties Participating in Ohio JOBS Demonstration

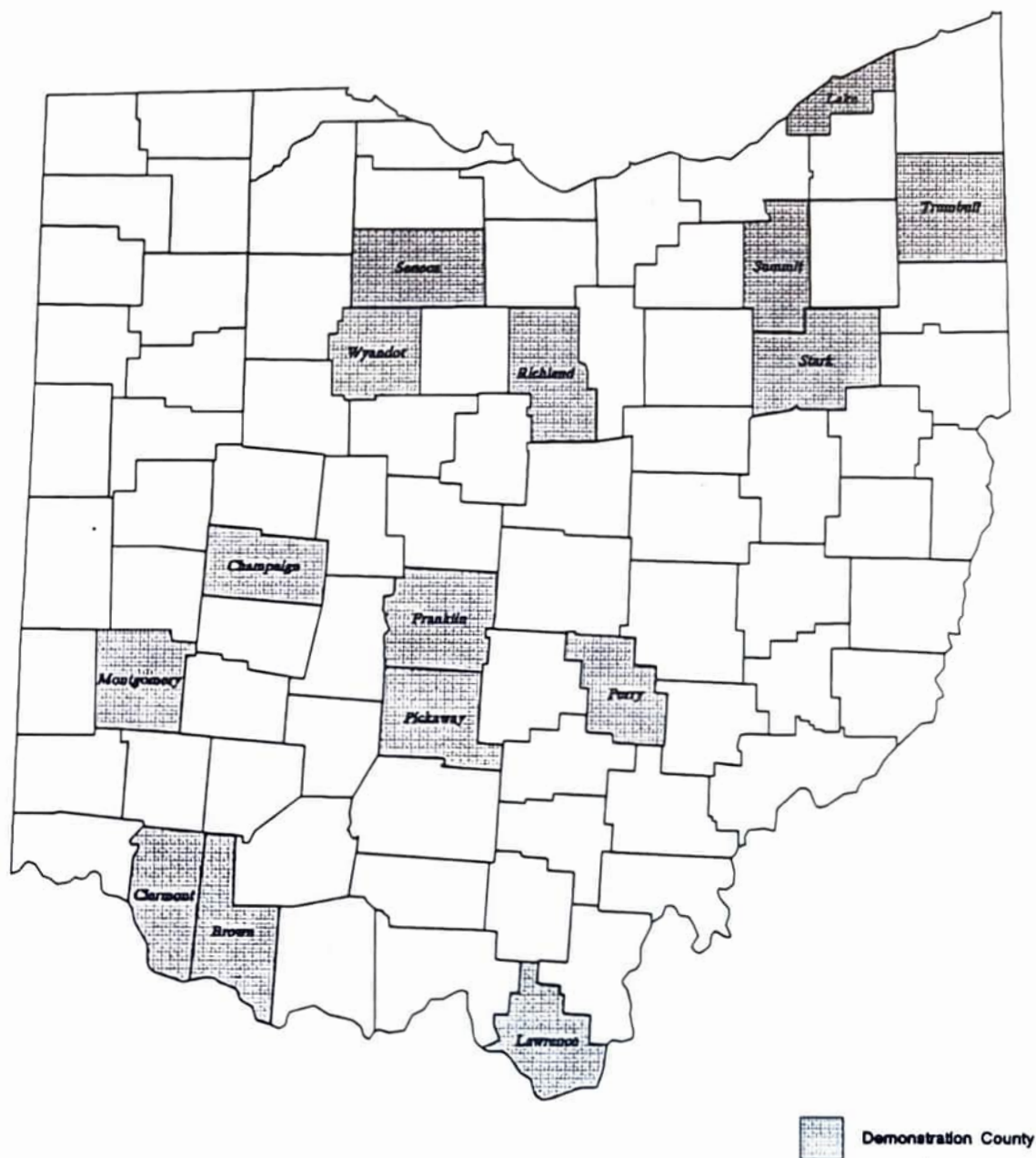


Exhibit 1.3

Social and Economic Characteristics of the Population in the Demonstration Counties, Ohio and the U.S.

Geographic Area	1990 Population	% of '90 Pop. in Metropolitan Statistical Areas	Percent of Workers in:				% of 1990 Population African-American	% of Families with Female Head in 1990	% of 1990 population Aged 25+ w/out HS Diploma
			Services	Manufacturing	Government	Agriculture			
Brown	34,966	100.0	26.5	28.7	2.6	4.5	37.8	1.2	12.2
Champaign	36,019	0.0	22.8	37.1	3.6	4.9	31.6	2.8	11.0
Clermont	150,187	100.0	26.9	24.1	2.4	1.8	44.9	0.9	12.8
Franklin	961,437	100.0	35.6	12.2	6.0	1.0	45.2	15.9	19.7
Lake	215,499	100.0	27.7	29.3	2.6	1.5	38.9	1.6	13.0
Lawrence	61,834	100.0	31.2	19.7	2.7	1.7	44.7	2.5	15.9
Montgomery	573,809	100.0	33.2	21.7	6.5	0.8	37.8	17.7	19.3
Perry	31,557	0.0	26.4	26.5	5.2	2.3	39.5	0.2	13.2
Pickaway	48,255	100.0	22.5	24.3	5.9	3.5	43.9	6.3	11.8
Richland	126,137	100.0	26.0	31.3	3.4	1.9	37.4	7.9	14.3
Seneca	59,733	0.0	28.4	32.2	1.7	3.8	34.0	2.0	12.7
Stark	367,585	100.0	29.9	26.8	2.2	1.2	39.8	6.8	15.0
Summit	514,990	100.0	32.7	23.0	2.8	0.8	40.7	11.9	17.7
Trumbull	227,813	100.0	27.9	32.3	2.3	1.4	36.2	6.7	15.7
Wyandot	22,254	0.0	24.2	34.7	2.0	6.3	32.9	0.1	11.6
All 15	3,432,075	95.6	31.8	21.6	4.3	1.3	41.0	11.0	17.0
Ohio	10,847,115	79.0	31.4	23.1	3.8	1.8	39.9	10.6	11.7
US	248,709,873	77.5	32.7	2.1	4.8	2.5	57.8	12.1	16.5
									24.8

Sources: U.S. Bureau of the Census (1992a, 1992b, 1993a, 1993b, 1993c, 1994a, 1994b).

Exhibit 1.3 also reveals that several characteristics commonly associated with welfare dependency tended to be concentrated in the larger metropolitan counties. Notably, the three largest counties also had the highest percentages of persons of African-American ancestry and families with a female head. However, high rates of poverty and ADC receipt were not confined to the largest counties. As shown in Exhibit 1.4, Brown, Lake and Perry all had proportions in poverty and on ADC that were substantially above average.

Taken together, the demonstration counties were substantially more urban than the State as a whole. Exhibit 1.4 shows that 96 percent of the population in demonstration counties lived in Metropolitan Statistical Areas in 1990, compared with only 79 percent of the State's overall population. Reflecting this greater urbanization, the prevalence of female-headed families is higher for the demonstration counties than for the State as a whole.

For virtually all other characteristics summarized in Exhibits 1.3 and 1.4, the demonstration counties in the aggregate resemble the State quite closely. Statistics for the distribution of workers by industry; percent African-American; percent of adults without a high school diploma; rates of ADC receipt, poverty and unemployment; and basic ADC case characteristics all support this generalization.

To what extent are Ohio's general and ADC populations similar to those in the rest of the nation? Statistics in Exhibits 1.3 and 1.4 suggest that around the start of the demonstration in 1989, the State's population closely resembled the nation as a whole in its metropolitan concentration, racial composition, and general economic conditions (e.g., unemployment and poverty rates). Compared with the U.S., Ohio workers were somewhat more likely to be employed in manufacturing jobs, and families were somewhat less likely to be headed by a single female. Another striking difference (not shown in exhibits) is that Hispanics comprise a much lower percentage of the Ohio population (1.3 percent) than of the U.S. population (9.0 percent).

Exhibit 1.4 shows that two-parent cases constituted a higher percentage of the ADC caseload in Ohio (11 percent) than in the nation as a whole, reflecting the fact that a number of states had not yet implemented a two-parent program or were offering such assistance on a time-limited basis. Other characteristics shown in the exhibit -- case size and grant amounts -- are reasonably close to those of welfare cases nationwide.

Exhibit 1.4

Poverty and ADC Caseload Characteristics for the Demonstration Counties, Ohio and the U.S.

Geographic Area	% of Population % of Population		Average 1989		ADC Cases in 1989		ADC Cases in 1989		ADC Cases in 1989		ADC Cases in 1989		ADC Cases in 1989		ADC Cases in 1989		ADC Cases in 1989	
	Receiving ADC in January 1989	in Poverty in 1989	Unemployment Rate	Total Cases In January 1989	% with Two Parents	Avg. No. Recipients Per Case	Single-Parent	Two-Parent	Single-Parent	Two-Parent	Single-Parent	Two-Parent	Single-Parent	Two-Parent	Single-Parent	Two-Parent	Single-Parent	Two-Parent
Brown	4.8	14.2	7.5	574	18.5	2.7	3.8		313	389								
Champaign	2.9	8.8	5.8	396	16.4	2.4	3.7		285	357								
Clermont	3.7	8.7	5.3	1,922	15.9	2.6	4.2		308	417								
Franklin	5.9	13.0	4.3	19,899	4.9	2.7	4.3		302	441								
Lake	2.0	4.9	5.1	1,595	12.7	2.5	3.9		301	390								
Lawrence	12.6	23.5	5.5	2,536	29.1	2.7	3.9		305	386								
Montgomery	6.3	12.6	5.1	13,079	5.2	2.7	4.1		317	413								
Perry	8.3	19.1	9.2	844	24.5	2.8	4.1		317	405								
Pickaway	4.2	12.1	6.1	714	20.4	2.5	3.8		295	391								
Richland	4.6	11.3	6.5	2,125	12.7	2.6	3.8		305	390								
Seneca	4.3	10.8	7.7	858	19.9	2.7	3.8		308	393								
Stark	5.3	11.1	6.1	6,832	12.5	2.6	3.9		304	356								
Summit	6.1	12.1	5.4	11,494	9.8	2.6	3.9		299	374								
Trumbull	6.0	11.4	7.1	4,715	16.5	2.7	4.0		307	392								
Wyandot	1.9	8.5	6.7	160	22.5	2.4	3.7		295	396								
All 15	5.6	11.9	5.3	67,743	9.8	2.7	4.0		305	394								
Ohio	5.8	12.5	5.6	223,129	10.9	2.7	4.0		312	398								
US	4.4	13.1	5.3	3,770,959	5.1	2.7	4.4		370	584								

Sources: Administration for Children and Families (1989), Committee on Ways and Means (1992), Ohio Department of Human Services (1989) U.S. Bureau of the Census (1992a, 1992b, 1992d), U.S. Bureau of Labor Statistics (1989)

In sum, the 15 demonstration counties represent a large, and reasonably representative, sample of Ohio counties. The chief difference is that the demonstration counties are somewhat more highly urbanized than the State as a whole.

Trends in Ohio's ADC Caseload and Economy

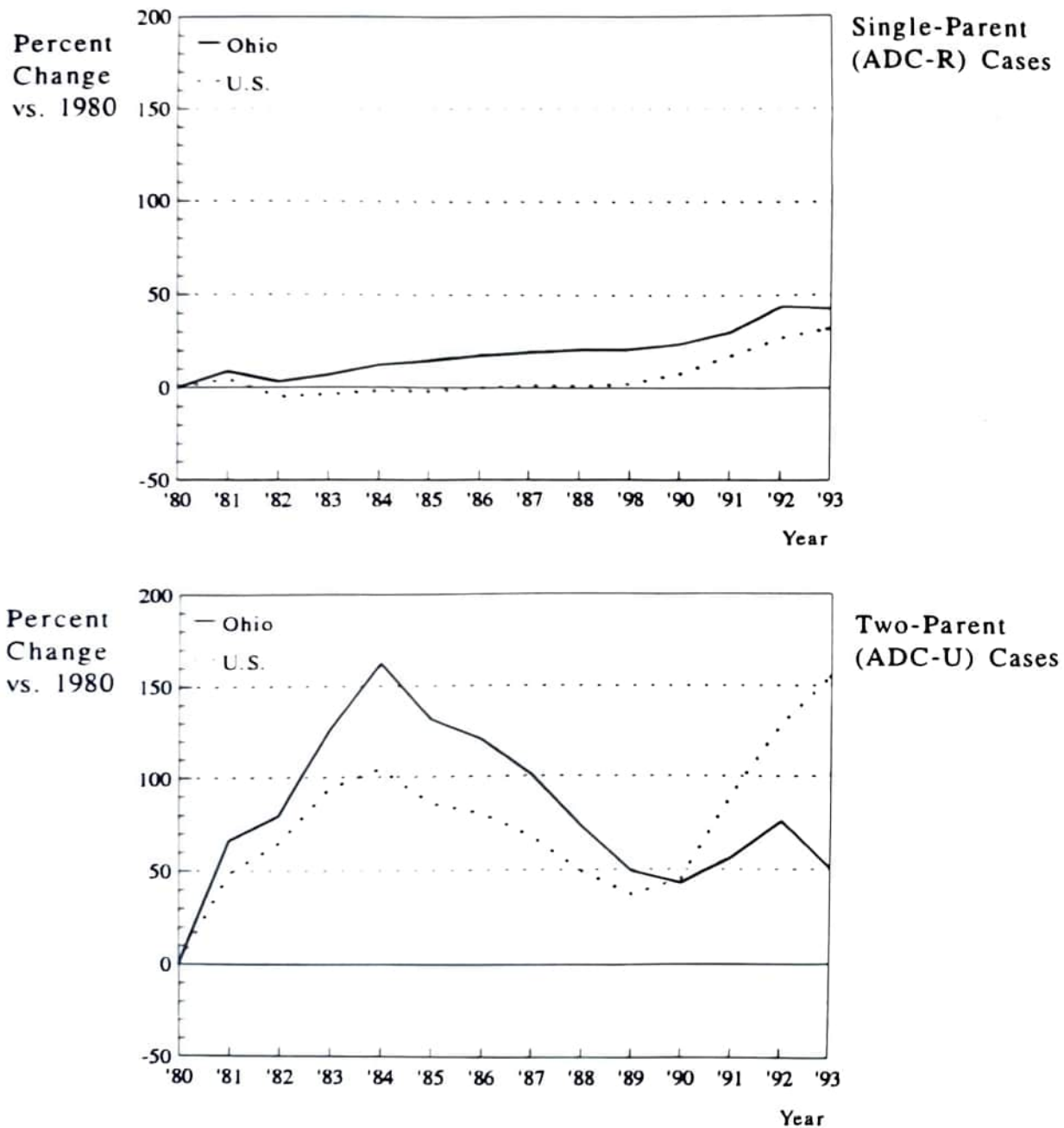
During much of the period covered by the demonstrations -- 1989 to 1992 -- ADC caseloads in Ohio were growing rapidly. Such growth indicates that local economic conditions were not optimal from the standpoint of placing JOBS participants in jobs. Caseload growth also places a direct burden on programs by increasing the number of clients who need services.

Exhibit 1.5 provides a broad historical look at trends in Ohio's ADC caseload from 1980 to 1992. In the early 1980's, Ohio's ADC caseload grew at a faster rate than that of the rest of the nation, reflecting the 1982-1983 recession's profound impact in the state (see Exhibit 1.6). Two-parent families were disproportionately affected, as they tend to have greater labor force attachment than single-parent families. Exhibit 1.5 shows that Ohio's two-parent (ADC-U) caseload increased by 150 percent between 1980 and 1984, whereas the single-parent (ADC-R) caseload grew by just 10 percent.

That the number of two-parent families applying for welfare is more sensitive to the economy than the number of single-parent families also is suggested by caseload changes between 1985 and 1989. Exhibit 1.5 shows that during this period, Ohio's ADC-R caseload continued its gradual increase, while the number of ADC-U cases fell by half. That the single-parent caseload continued to increase in the face of an improving economy most likely reflects secular trends associated with the long-term rise in female household headship. Changes in the ADC-U caseload closely tracked the substantial decline in unemployment rates during this period (Exhibit 1.6).

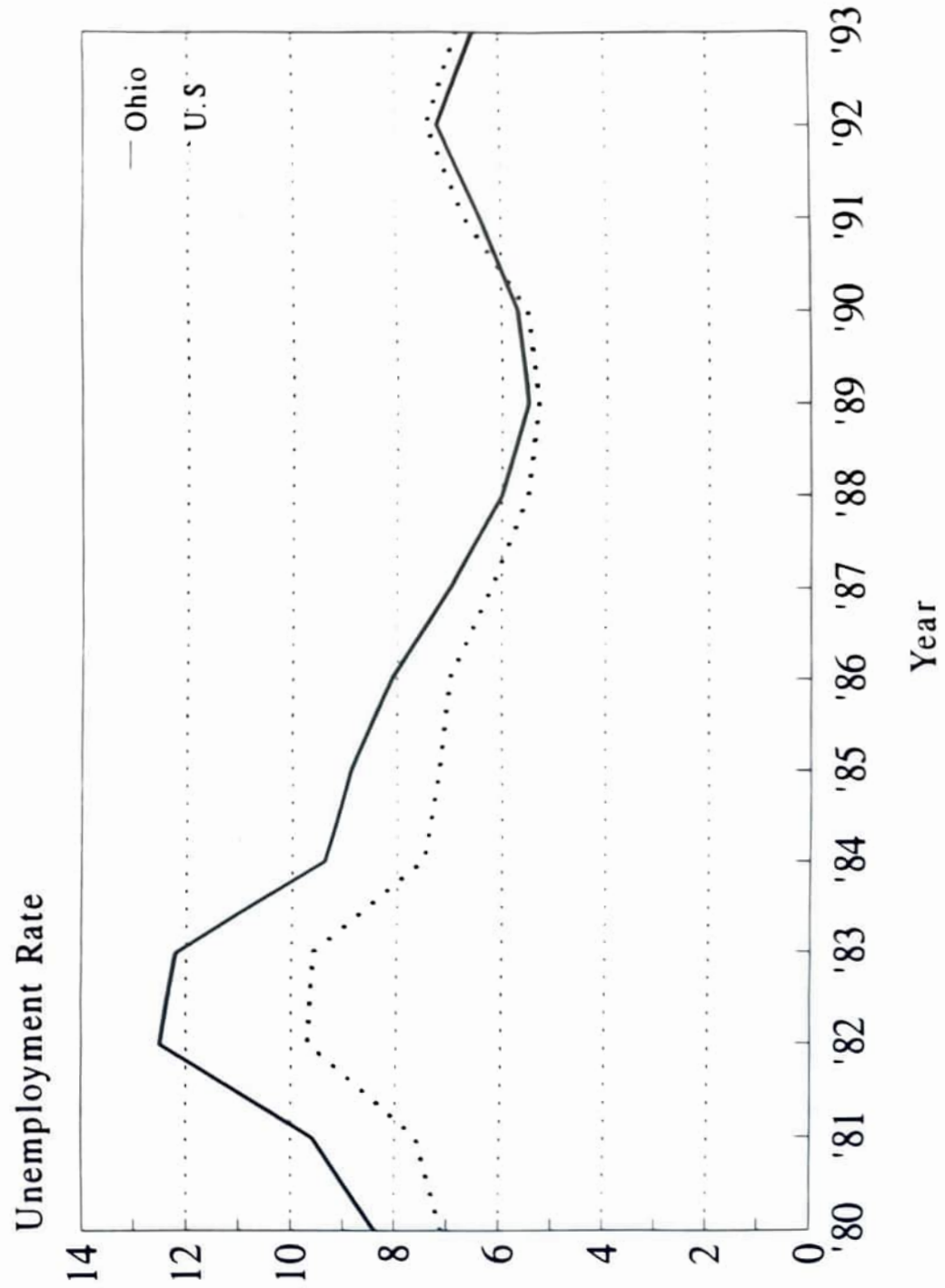
Entering the period of the JOBS and Work Choice demonstrations, economic recession again drove caseloads sharply higher. The proportionate increase among single-parent cases was especially large by historical standards. The painful economic restructuring of the early 1980's resumed, with the loss of an additional 72 thousand manufacturing jobs in the State between 1989 and 1992. Although employment in services increased, ADC clients likely faced stiff

Exhibit 1.5
Percentage Change in AFDC Caseload Relative to 1980:
Ohio and the U.S.



Source: U.S. Administration for Children and Families (1980-1993)

Exhibit 1.6
Average Annual Unemployment Rates for Ohio and the U.S.
1980-1993



Source: U.S. Bureau of Labor Statistics (1993)

competition for these openings from more experienced workers who had lost jobs in other industries.

The recession and associated caseload increases lasted through most of the demonstration period (1989-92). With the downturn in unemployment late in 1992, Ohio's ADC-U caseload began another sharp decline.²¹ For the first time since 1982, a slight decline among ADC-R cases also was registered in 1992.

In sum, neither economic nor caseload trends provided a very favorable environment for launching Ohio's JOBS program. A difficult economy faced ADC clients seeking work for much of the demonstration period. As if this was not a daunting enough problem, each year JOBS staff faced increasing caseloads of clients to serve.

1.6 SUMMARY

This chapter has described the Ohio JOBS and Work Choice demonstrations and the social and economic environments in which they operated. Such information is an important prerequisite for understanding the sources of program impacts and for making appropriate comparisons with findings from other demonstrations.

Hallmarks of Ohio's JOBS program include: county-based administration of key program services; an assignment model providing flexibility to caseworkers and clients in choosing JOBS activities; an overall emphasis on intensive educational services; and the placement of a relatively high fraction of participants at CWEP sites, often as an assignment of last resort.

Two distinctive aspects of the research design used to measure the impacts of JOBS and Work Choice bear repeating here. First, sample enrollment began at the same time counties first were implementing their programs. Second, individual clients entered the sample at the point that they first were determined to be JOBS-mandatory, regardless of when they first (or whether they ever) received JOBS services.

²¹ Economic trends explain only part of the U.S. growth in two-parent cases between 1989 and 1992. During this period, many states that had not previously served unemployed parents were extending AFDC eligibility to two-parent families, as the 1988 Family Support Act required. It seems likely that most, if not all, of the excess growth in two-parent cases nationally compared with Ohio (which long had operated its unemployed parent program) reflects this policy change.

An advantage of this design is that it provides estimates of program effects that clearly pertain to the entire early program implementation period: impacts from designs which delay sample enrollment until some time after program start-up characterize a somewhat less clearly-defined stage of program development. The chief shortcoming of the Ohio design is that it may understate what would be achieved if the same programs were up, running, and reaching a high fraction of their intended participants.

The 15 counties participating in the JOBS demonstration provided a diverse mixture of social and economic conditions in which to test JOBS. On the whole, the counties were somewhat more urban than either Ohio or the U.S. as a whole. Nonetheless, the demonstration counties did resemble the State and the nation in many respects.

The recession which drove up caseloads nationally between 1989 and 1992 had a similar effect in Ohio, greatly magnifying the difficulties associated with implementing JOBS. The recession meant that local programs struggling to build basic service capacities also had to contend with tight local job markets and a substantial increase in the number of clients to be served.

Having provided background on the JOBS and Work Choice demonstrations, we now turn to our empirical findings. Chapter 2 presents an analysis of the characteristics of demonstration participants and their participation in employment and training activities. Chapters 3 and 4 present impact findings for the JOBS and Work Choice demonstrations, respectively. Chapter 5 offers our overall conclusions.

CHAPTER TWO

CHARACTERISTICS AND PROGRAM EXPERIENCES OF DEMONSTRATION ENROLLEES

In this chapter, we summarize the characteristics and employment and training experiences of ADC clients enrolled in the JOBS and Work Choice demonstrations. Past studies have found that the impacts of welfare-to-work programs can vary greatly with the age, education, time on welfare, previous employment experience, and other characteristics of participating ADC clients. The distribution of such attributes varies across demonstrations both because characteristics of overall ADC caseloads vary geographically and because evaluations may target different segments of the caseload. Documenting the characteristics of demonstration participants is an important first step in placing study findings in their proper context.

In examining the characteristics of demonstration participants, we focus on attributes measured at "baseline," the point that exposure to JOBS or Work Choice begins for members of the treatment group (or would have begun for control-group members, had they instead been assigned to the treatment group). For treatment-group members in each demonstration, baseline indicates the point that clients first became subject to new requirements and eligible for new services *in theory*, regardless of when their first meaningful program experiences actually occurred. Because baseline precedes program participation, we can be confident that the portrait that is drawn accurately reflects clients' starting attributes and endowments.

Describing the essence of the "treatment" received by participants is a second crucial evaluation task. As noted in the last chapter, the national JOBS program permits great variation in the level and nature of employment and training assignments across states and localities. In evaluating impacts for Ohio's JOBS program it is vital that the program's distinctive emphases be clearly identified. Given its voluntary nature, Work Choice could generate participation patterns quite different from JOBS; these patterns also warrant attention.

The first section of this chapter summarizes the characteristics of clients in the Ohio JOBS demonstration, and the overall level and nature of employment and training assignments

they received. The second section presents similar information for clients in the Work Choice sample.

2.1 JOBS

JOBS regulations allow states to require participation for all ADC recipients except those in certain exemption categories. The nature and extent of exemptions suggest that the characteristics of JOBS-mandatory clients are likely to differ significantly from those of the overall caseload.¹ In particular, since the most important exemption by far was for being the caretaker of a child under age six (under age three after June 1991), JOBS-mandatory clients will tend to have characteristics associated with families without young children.²

Within the population of mandatory clients, characteristics and assignment patterns also are likely to vary by year of demonstration entry. The fact that cohorts differed in the clients' average length of ADC receipt at demonstration entry accounts for much of this variation. At demonstration entry, a majority of 1989 and 1991 cohort were in the midst of ongoing spells of ADC receipt. In contrast, a majority of the 1990 cohort was just starting ADC spells at demonstration entry. Ongoing cases represented a majority of 1989 entrants because existing caseloads in the 15 demonstration counties accounted for most new JOBS-mandatory clients during that year. As Franklin County did not begin JOBS for its large ADC-R caseload until January 1991, a large population of existing cases also is found in the 1991 cohort.

¹ More specifically, ADC recipients are exempt from Ohio's JOBS program if they are: children under 16; children between 16 and 18 attending school full-time; ill or incapacitated, or needed to care for an ill or incapacitated household member; at least 60 years old or in a nursing home; a parent caring for a child under three; residing in a remote area of the state; working 30 hours or more per week; pregnant women, beginning with the third month of pregnancy; participating in the LEAP program; a full-time VISTA volunteer. Only one parent in an ADC-UP family can be exempted for caring for a child under three. Both parents can qualify for any of the other exemptions. Prior to July 1991, Ohio's JOBS program also exempted caretakers of children aged three to five.

² In FY 1991, 72 percent of Ohio exemptions were related to child care (U.S. Department of Health and Human Resources n.d.).

Exhibit 2.1

Characteristics of Clients in the JOBS Sample at Demonstration Entry

Characteristic	Year of Demonstration Enrollment ^a		
	1989	1990	1991
Demographic			
Age			
Under 25 Years	12.7%	23.2%	26.5%
25-39 Years	68.5	63.5	63.3
40+ Years	18.8	13.3	10.2
Race			
White	76.5%	76.9%	62.5%
Non-White	23.5	23.1	37.5
Gender			
Female	62.1%	58.4%	81.1%
Male	37.9	41.6	18.9
Family Type			
Single-Parent (ADC-R)	62.4%	56.9%	82.2%
Two-Parent (ADC-U)	37.6	43.1	17.8
Number of Children			
Less than 3	74.4%	75.8%	76.1%
3-4	23.0	21.7	21.6
5+	2.6	2.5	2.3
Mean	2.0	1.9	1.9
Socioeconomic			
Completed Years of Education ^b			
Less than 12 Years	44.2%	-	-
12 Years	44.1	-	-
13+ Years	11.8	-	-
Ever Employed in Six Months Before Demonstration Enrollment?			
No	71.2%	60.0%	61.5%
Yes	28.8	40.0	38.5
Duration of ADC Receipt in Current Spell			
New/Re-Opened Case	33.8%	58.4%	35.0%
Less than Two Years	22.2	25.6	33.6
Two Years or More	44.0	15.9	31.4
Number in Sample ^c	8,485	4,674	12,976

^a A chi-square test indicated that the distribution of every characteristic in each cohort is significantly different from the remaining sample (the other two cohorts combined) at the 5% significance level, with one exception: the distribution of number of children for the 1990 cohort.

^b Not available in extract files for significant proportions of 1990 and 1991 enrollees due to conversion to new computer system (CRIS-E).

^c Includes both treatment and control group clients.

The Extent of Participation

Exhibit 2.2 shows the time patterns for assignments to JOBS employment and training activities -- excluding assessments -- over the follow-up period. The lines in the chart show the cumulative percentages of treatment-group members in each cohort who ever were assigned to one or more JOBS activities over increasing intervals of follow-up. Lines for the 1990 and 1991 cohorts are progressively shorter than those for the 1989 cohort because the follow-up periods are shorter.

The graph shows that most clients did not receive a JOBS assignment during the follow-up period. After three full years, only about 42 percent of 1989 enrollees had ever taken part in a JOBS activity.

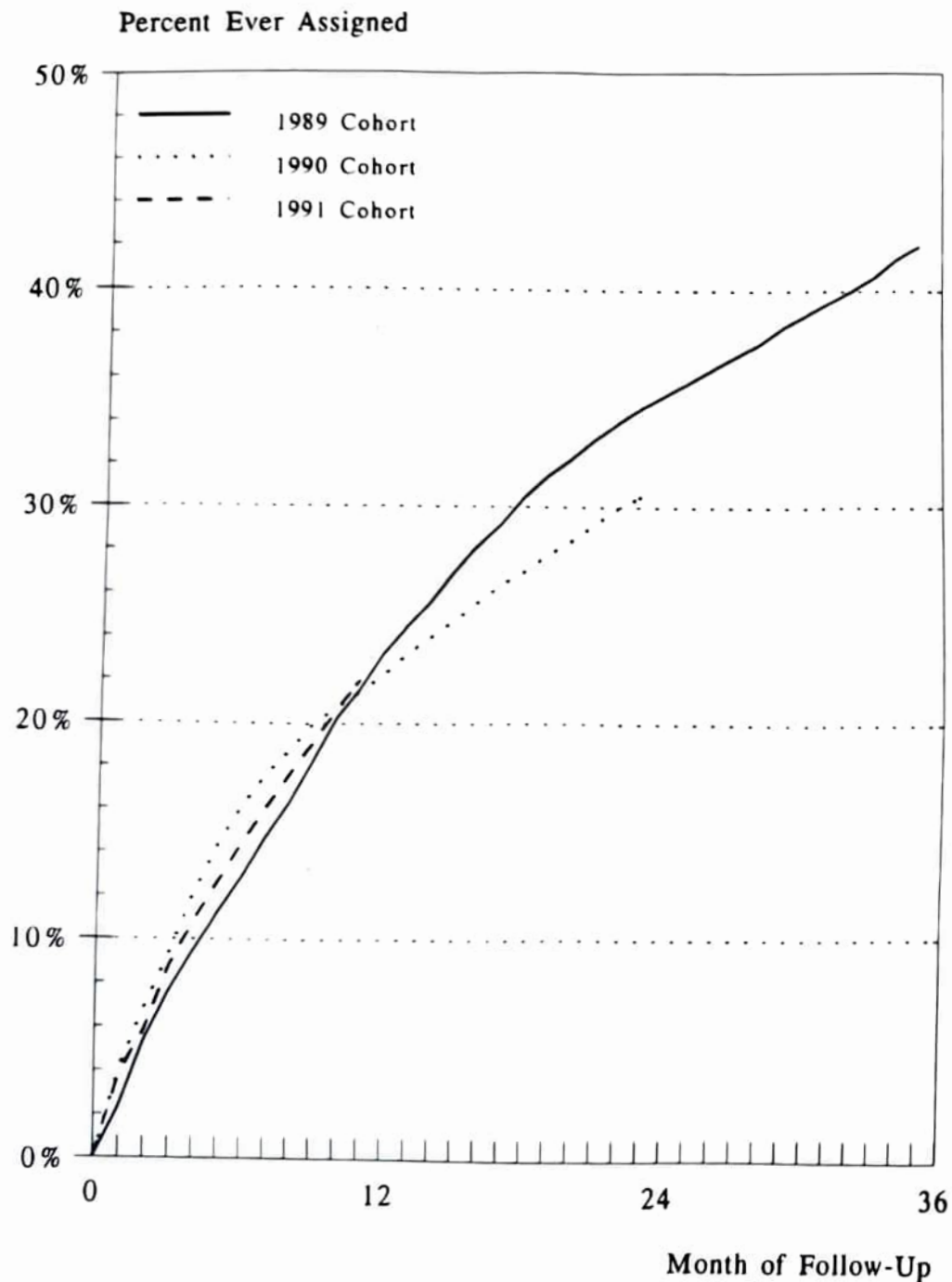
Non-assignment is traceable to a number of factors, of which insufficient program staffing and placement resources are only one. Many clients' participation was intentionally deferred for medical, child care, or transportation-related reasons.⁴ Another sizeable portion likely left ADC before there was a chance to serve them. The slow processing of clients into many counties' JOBS programs that characterized much of the demonstration period increased the chances that clients would leave welfare before they could receive JOBS services. Although such delays were unintentional, they may have had the serendipitous effect of concentrating JOBS resources on the clients least able to find their own ways off public assistance.

Exhibit 2.2 suggests that the wait to first assignment often was lengthy for those who eventually were assigned. For example, fewer than 25 percent of 1989 cohort members were assigned during the first year of follow-up, although 42 percent eventually received assignments.

Ever-assignment rates provide a useful gauge of JOBS' potential impacts, since they indicate the fraction of the population that was exposed to its central services. In this light, it should be noted that rates of ever-assignment in Ohio during the demonstration period were low compared to those found in recent evaluations of the Florida and California JOBS programs. In the first year of follow-up, over half of the treatment-group members in California and 43 percent of those in Florida received JOBS employment and training assignments, compared to

⁴ Unfortunately, assessments and deferrals were recorded in automated systems too inconsistently over the demonstration period to allow us to estimate their contribution to nonparticipation.

Exhibit 2.2
Percent of JOBS Treatment Group Members Ever Assigned
to Employment and Training Activities
by Month of Follow-Up and Cohort



only about 22 percent in Ohio (Kemple and Haimson 1994; Riccio *et al.* 1994)⁵. If all other things were equal, these lower rates would create an expectation of smaller impacts.

Although useful, rates of ever-assignment should not be confused with more conventional participation measures which divide the average number of months clients spent in assignments into the average number of months they were JOBS-mandatory in a given time period. The distinction is especially important to bear in mind when interpreting differences in ever-assignment across different subpopulations, for which the average length of time JOBS-mandatory may have varied.

For example, differences in time spent as JOBS-mandatory after demonstration entry largely explain the puzzle of why a lower fraction of 1990 than of 1989 cohort members had received assignments after two years of follow-up, even though start-up difficulties had eased by the second year of the demonstration. Because they were disproportionately new recipients, the 1990 cohort tended to leave ADC faster than 1989 cohort and thus spend less time exposed to assignment.⁶

Assignment Patterns

In addition to the level of participation, it is important to look at the types of activities to which JOBS participants were assigned. Exhibit 2.3 shows patterns of assignment by cohort. For each activity, the table gives the assignment rate for *all* treatment-group members, and for the subset of treatment-group members who were *ever assigned* to any activity. Clients with multiple assignments are counted once for each activity in which they participated.

The results suggest that Ohio's JOBS program has emphasized activities, such as basic and post-secondary education, which take a relatively long time to complete. Overall, more than half of ever-assigned clients received basic or post-secondary education assignments. In

⁵ Higher reported assignment rates for California and Florida here likely are a result of the fact that these demonstrations enrolled more narrowly-targeted segments of their JOBS-mandatory caseloads than Ohio. Whereas Ohio enrolled all mandatory clients in most counties, California enrolled only clients who appeared at an initial GAIN orientation session. Florida enrolled only clients just starting welfare spells and existing clients who became newly-mandatory during the enrollment period.

⁶ Comparison of Exhibits 3.3 and 3.4 shows that 1990 cohort control group members averaged fewer months of ADC participation than their counterparts in the 1989 cohort.

Exhibit 2.3

**Patterns of Assignment to Employment and Training Activities
for Clients in the JOBS Treatment Group**

Activity	Percent of <u>All</u> Clients Ever Assigned to Activity ^{a, b}	Percent of <u>Ever</u> <u>Assigned</u> Clients Assigned to Activity ^a
<u>1989 Cohort</u>		
Basic Education	12.0	29.6
Post-Secondary Education	7.7	19.0
Job Skills Training	1.6	3.9
Job Readiness Training	4.7	11.6
Job Club	9.0	22.2
CWEP	19.1	47.3
Any Activity	40.5 ^{c, d}	100.0 ^c
Number of Clients Observed	7,860	3,184
<u>1990 Cohort</u>		
Basic Education	9.8	33.1
Post-Secondary Education	7.0	23.9
Job Skills Training	1.6	5.3
Job Readiness Training	4.4	14.9
Job Club	6.1	20.7
CWEP	9.4	32.0
Any Activity	29.4 ^{c, d}	100.0 ^c
Number of Clients Observed	5,239	1,542
<u>1991 Cohort</u>		
Basic Education	8.4	37.6
Post-Secondary Education	5.7	25.4
Job Skills Training	1.1	5.1
Job Readiness Training	3.9	17.5
Job Club	5.0	22.4
CWEP	5.4	24.3
Any Activity	22.4 ^{c, d}	100.0 ^c
Number of Clients Observed	11,021	2,465

^a Further explanation of how rates were calculated is provided in notes to Exhibit ES.1.

^b These percentages decline across entry years in part because follow-up periods become shorter.

^c Sums for activities do not equal totals for Any Activity because some clients had multiple assignments.

^d Percent assigned to Any Activity is slightly different from the percent ever assigned in Exhibit 2.2 because different methods were used to count sample members who migrated out of the demonstration counties.

contrast, there was a relatively low emphasis on less intensive, short-term activities such as job search. Ohio's emphasis on activities with a long time horizon means that positive impacts may take longer to surface here than in programs which emphasize immediate employment. Ohio also made extensive use of CWEP, an activity of moderate length.⁷

Exhibit 2.3 reveals several changes in assignment emphases across enrollment years. The biggest changes over time were an increase in the use of both basic and post-secondary education, and a decrease in the use of CWEP. The use of job club did not change much. The effect of the changes was to increase Ohio's already strong emphasis on strategies with long time horizons.

It is instructive to compare JOBS assignments in Ohio with those in California and Florida, since impacts from rigorous evaluations also have been reported for these states. Recently released federal statistics based on the ACF-108 JOBS reporting system (U.S. Department of Health and Human Services 1994) are useful because they standardize definitions for different types of activities. The ACF-108 data reveal the following:

- Ohio's emphasis on basic education was intermediate between those of Florida and California;
- Job search accounted for a much higher proportion of Florida JOBS participants than Ohio or California participants;
- Ohio had the highest percentage of participants in post-secondary education programs (assigned and self-initiated), whereas California had the highest fraction in vocational and job skills training; and
- Ohio had by far the highest percentage of participants in CWEP.

⁷ Fein (forthcoming) presents estimates of the median length of clients' first assignments to different JOBS activities in Ohio. Median spell lengths were: 8.8 months for post-secondary education; 8.5 months for basic education; 5.2 months for job skills training; 5.2 months for CWEP; 4.1 months for job readiness; and 1.7 months for job club. It should be noted that these estimates may have an upwards bias, given that case workers did not always record activity end dates on a timely basis. Nonetheless, the estimates probably provide a valid general portrait of differences in spell lengths across activities.

Assignment Patterns for Subgroups

The national JOBS program contains targeting provisions and employment and training activity emphases that lead to the expectation that both the overall level and mix of assignments will vary for different client subgroups. Documenting this variation in services provides important evidence which may help in interpreting any differences in program impacts across the subgroups.

Exhibit 2.4 shows assignment patterns for a number of subgroups within the 1989 cohort. The exhibit's first row gives the percentage of each subgroup whose members ever received assignments over the three-year follow-up period. The remaining rows show the percentages of ever-assigned clients in each subgroup who were assigned to different employment and training activities.

The table shows clear differences in assignment rates across subgroup categories. Clients with no recent work experience, as well as clients in long ADC spells, have significantly higher participation rates than clients with recent work experience and those in new cases. The primary reason for these differences in assignment rates is probably differences in average stay on ADC. For example, clients with no recent work experience are likely to be more disadvantaged than recipients with work experience, and therefore are likely to have longer stays on ADC. The longer the stay on ADC, the greater the probability of being assigned.

Differences in time on ADC also can shed light on ever-assignment rates that seem to be similar, such as those for clients from single- and two-parent cases in Exhibit 2.4. Two-parent cases tend to have shorter ADC spells, suggesting that while clients were JOBS-mandatory their participation rates actually were higher than those for single-parent cases.

The second panel of Exhibit 2.4 reveals several differences in the types of assignments received by different subgroups that are worth noting. The largest differences are seen in the percentage of clients assigned to CWEP. In general, clients likely to have less work experience -- such as those who had not worked in the six months preceding demonstration enrollment and those who had been on ADC for the longest time -- were substantially more likely to receive CWEP assignments than those with more work experience. An exception was that a higher percentage of clients from two-parent families were assigned to CWEP than clients from single-

Exhibit 2.4
Assignments to Employment and Training Activities for 1989
Demonstration Entrants in Selected Subgroups

Activity	Ever Employed in 6 Months Before Demonstration Enrollment?		Duration of ADC Receipt in Current Spell			Family Type	
	No	Yes	New/Re-opened Case	Less Than 2 Years	2 Years or More	Single-Parent	Two-Parent
Percent of <u>All</u> Clients Ever Assigned to Any Activity	42.9***	32.2	32.6***	37.4	47.3	39.5	41.0
Percent of <u>Ever-Assigned</u> Clients Receiving Assignment*							
Basic Education	30.5**	26.5	29.1	28.4	30.3	32.3***	26.3
Post-Secondary Education	18.5	20.2	16.8**	21.8	18.8	23.3***	13.6
Job Skills Training	3.7	4.5	3.9	4.9	3.5	3.6	4.2
Job Readiness Training	10.2***	16.4	14.4**	10.4	10.6	14.1***	8.6
Job Club	20.7***	28.1	23.7**	25.8	20.4	20.5***	24.7
CWEP	49.9***	37.6	40.9***	46.6	50.4	41.3***	54.1
Total Number in Treatment Group	4,738	3,122	2,629	1,752	3,479	4,366	3,494

* Sums for activities do not equal 100 percent because some clients were assigned to more than one type of activity

* Assignment rates for subgroup categories statistically different from each other at the 10% level; ** at the 5% level; *** at the 1% level.

parent families: the former usually have greater work experience because of the additional work history requirements imposed on two-parent families for ADC eligibility.⁸

As could be expected, clients who had worked sometime during the six months prior to demonstration entry were more likely to receive job search and job readiness assignments than those who had not. At the same time, many clients with recent work experience did receive assignments to basic and post-secondary education. Intensive educational assignments were more common for clients from single-parent cases than for those from two-parent cases, although, again, assignments to educational activities was not uncommon among the latter.

Differences in employment and training emphases across subgroups should not be overstated. Although these differences are statistically significant, their magnitudes are not large enough to suggest fundamental differences in service approaches between subgroups. The percentage of clients receiving particular assignments may have varied across groups, but assignments' rankings -- relative to other assignments -- were quite stable.

Use of Sanctions in JOBS

In addition to the mix of program services provided, another feature that distinguishes JOBS programs is the extent to which they relied on sanctions to enforce participation (Lurie and Hagen 1993). The Family Support Act provides states with the authority to sanction JOBS-mandatory clients who, without good cause: fail to participate; refuse to accept employment; terminate employment; or reduce their work effort. One reason why state approaches to sanctioning vary is that participation standards have been low enough to allow states to meet the standards by relying largely on voluntary participation.

Ohio's JOBS program used sanctions during the demonstration period, but not extensively. Over all enrollment years, 4.2 percent of the treatment-group members in our sample were sanctioned. About two-thirds of all sanctions were imposed for failure to report

⁸ This apparent discrepancy between the need for and assignments to work experience among two-parent families may reflect Ohio's effort to satisfy the strong emphasis on work-related activities for unemployed parents which is contained in the national JOBS regulations. Although penalties for failure to assign a minimum percentage of clients from unemployed parent cases to work activities did not take effect until FY 1993, JOBS rules emphasized such assignments early on.

for assessment. The majority of the other sanctions were imposed for failure to participate in a JOBS activity.

Summary

The profile of the typical JOBS participant in Ohio during the demonstration period is that of a single white female between 25 and 39 years old, with two children and 12 years of school. For certain characteristics there are substantial differences across enrollment years. Because 1989 enrollees were more likely to come from existing cases, and 1990 entrants from new cases, the former tended to be older and exhibit greater welfare dependency (as measured by ADC spell length at entry and recent work experience). The 1991 cohort was disproportionately made up of single-parent cases from Franklin County, which did not begin to enroll its ADC-R caseload in JOBS until January 1991.

The early years of Ohio's JOBS program saw relatively low participation rates and long average waits to first assignment. Most of the JOBS-mandatory clients who enrolled in 1989 did not participate in any activity within three years of enrollment. Although Ohio's participation rates exceeded federal standards throughout the demonstration period, they were low compared to the two other demonstrations for which comparable data are available (California and Florida).⁹

In terms of the mix of services offered, Ohio's program has tended to stress education and downplay job search, giving it a relatively long-term focus. Ohio also made extensive use of CWEP. Over the course of the demonstration period, assignments to CWEP declined and those to education increased.

⁹ The extent to which differences between assignment rates for *demonstration participants* across the three states are generalizable to the entire mandatory populations of California, Florida, and Ohio are unclear. When one looks at official JOBS participation rates, for instance, one does not find that Ohio is at a great disadvantage. In FY 1992, HHS reported JOBS participation rates of 12 and 15 percent for California and Florida, respectively, compared with 19 percent for Ohio. Differences in state JOBS participation figures must be interpreted with great caution, owing to the problematic nature of reporting during the program's early years. Nonetheless, the possibility is quite strong that participation patterns for the three states overall are not so different as is indicated by analyses only of the populations participating in the JOBS demonstrations in these states.

The evidence on participation would lead us to expect smaller impacts for Ohio's JOBS program than for the other large-scale JOBS demonstrations in California and Florida. Ohio's participation rates were lower, and there was less emphasis on job search, an activity expected to generate impacts in the short term. Ohio's program might work well for those who took part, but the overall effects will be diluted by including the majority of enrollees who did not participate.

2.2 WORK CHOICE

As described in Chapter 1, the Work Choice demonstration operated in Montgomery County between January 1989 and March 1990. The program served single-parent ADC recipients whose youngest child was between one and five years old, a portion of Ohio's ADC caseload that was not originally mandatory for JOBS.¹⁰ The Work Choice program consisted of mandatory employability assessment and counseling, voluntary participation in education and training activities, and an offer of up to 12 months' eligibility for transitional Medicaid and child care payments for clients who left ADC for work.

The demonstration population can be divided into two cohorts based on when clients first became eligible for Work Choice services (or, in the case of control-group members, when they would have become eligible if they had been assigned to the treatment group). Clients entering the Work Choice demonstration during its first six months (January through June 1989) were mainly from ongoing ADC cases, whereas those entering after the sixth month were mostly clients who were starting new ADC spells. Because observation of all clients in the Work Choice sample ended in December 1990, longer follow-up was available for earlier demonstration entrants.

The two cohorts differ in two important respects beyond the amount of follow-up that was possible. One was their sociodemographic composition. As in the JOBS demonstration, early entrants tended to be older, and have fewer children and less work experience than later

¹⁰ Starting in July 1991, the child care exemption for JOBS in Ohio was restricted to care for children under age three, as in the national JOBS rules.

entrants. A second important difference was in the nature of the policies experienced by early and later entrants. The demonstration changed markedly after March 1990, when, as required by the Family Support Act, the State extended eligibility for transitional child care and Medicaid benefits to all ADC recipients leaving the rolls for employment, including Work Choice control-group members. Hence, control-group members among later Work Choice demonstration entrants experienced the original (more restricted) transition benefits for a much shorter time interval than did earlier entrants.

Characteristics of the Work Choice Population at Baseline

The fact that Work Choice targets a different population from JOBS -- ADC-R clients with young children -- and was confined to one large urban county implies that the characteristics of clients in the Work Choice demonstration will be quite different from those of the JOBS sample. Exhibit 2.5 reveals this indeed to be the case. Clients with young children tend to be much younger than those with older children, as suggested by the high percentage of Work Choice clients under age 25 (42 to 49 percent), compared with JOBS clients (13 to 27 percent). Due to the large African-American population in Montgomery County, a majority (55 to 58 percent) of members of the Work Choice sample was non-white, compared with much lower percentages (23 to 37 percent) of the JOBS sample. Work Choice sample members also were more likely to be long-term ADC recipients than JOBS sample members. Finally, whereas all Work Choice eligibles were female, sizable fractions (18 to 43 percent) of the JOBS sample were male.

As was true for the mandatory JOBS population, the two Work Choice entry cohorts differed from one another on many characteristics shown in Exhibit 2.5. Most notably, members of the early cohort were older and had received welfare longer on average than later enrollees. Fifty-eight (58) percent of early demonstration entrants were 25 years or older, compared with 51 percent of later entrants. Fifty-seven (57) percent of early entrants had received ADC continuously for two years or more at baseline, compared with only 41 percent of later entrants. The early cohort also was less likely to have worked during the six months preceding sample entry (21 percent) than the later cohort (29 percent).

Exhibit 2.5

Characteristics of Clients in the Work Choice Sample at Demonstration Entry

Characteristic	Period of Demonstration Entry	
	Jan. 1989 - June 1989	July 1989 - March 1990
Demographic		
Age ^{***}		
Under 25 Years	42.4%	48.8%
25-39 Years	54.8	48.8
40+ Years	2.8	2.4
Race		
White	44.6%	42.4%
Non-White	55.4	57.6
Number of Children		
Less than 3	70.9%	68.6%
3-4	25.5	28.2
5+	3.6	3.2
Mean	2.1	2.1
Number of Children Aged 1-5 Years ^{***}		
1	69.3%	58.5%
2	24.7	31.4
3+	6.0	10.1
Socioeconomic		
Completed Years of Education		
Less than 12 Years	43.3%	43.1%
12 Years	44.5	43.8
13+ Years	12.2	13.1
Ever Employed in Six Months Before Demonstration Enrollment? ^{***}		
No	79.4%	70.8%
Yes	20.6	29.2
Duration of ADC Receipt in Current Spell ^{***}		
New/Re-Opened Case	11.4%	25.6%
Less than Two Years	31.2	33.0
Two Years or More	57.4	41.4
Any Relative on ADC? [*]		
No	43.1%	40.1%
Yes	56.9	59.9
Number in Sample [*]	4,265	1,344

* Includes both treatment and control group clients.

A chi-square test was performed to detect differences in each characteristic's distribution between cohorts:

* Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

Cohort differences arise from sources similar to those in the JOBS sample. At the beginning of the demonstration there was a large backlog of Work Choice-eligible ADC recipients who were older, longer-term welfare recipients who tended to have less recent work experience. These individuals represented the vast majority of enrollees during the first six months of program operations. Later entrants were disproportionately drawn from the ranks of clients beginning new welfare spells.

Participation in Employment and Training Activities

This section analyzes the level and patterns of participation in employment and training activities by clients in the Work Choice treatment group. In addition to differences in the types of clients eligible, we expect that the voluntary nature of employment and training participation under Work Choice might lead to very different patterns from JOBS. Again, we look at participation separately for early and later demonstration entrants.

Exhibit 2.6 shows the cumulative percentage of enrollees in each cohort which had been assigned to employment and training activities after successive months of follow-up. By 12 months, 8-9 percent of Work Choice-eligible clients had volunteered for at least one activity; by 18 months, about 14 percent had volunteered. The findings suggest that slightly higher assignment rates were sustained for later than for earlier demonstration entrants during their first year in the program.

Although these rates may seem low, it should be noted that they are roughly half as large as those seen in Exhibit 2.2 for mandatory JOBS clients at similar exposure intervals. The significance of this comparison may lie more in what they reveal about the difficulties in making the early Ohio JOBS program truly mandatory than in any novel formula Work Choice used to encourage voluntary employment and training participation. Nonetheless, the Work Choice assignment rates are high enough to suggest that the employment and training component of this demonstration might have some impacts on self-sufficiency.

Perhaps more interesting are differences in the nature of assignments between Work Choice and JOBS. The second column of Exhibit 2.7 shows that Work Choice clients overwhelmingly opted for education and training assignments. Work-related activities -- job

Exhibit 2.6
Percent of Work Choice Treatment Group Members Ever Assigned
to Employment and Training Activities
by Month of Follow-Up and Cohort

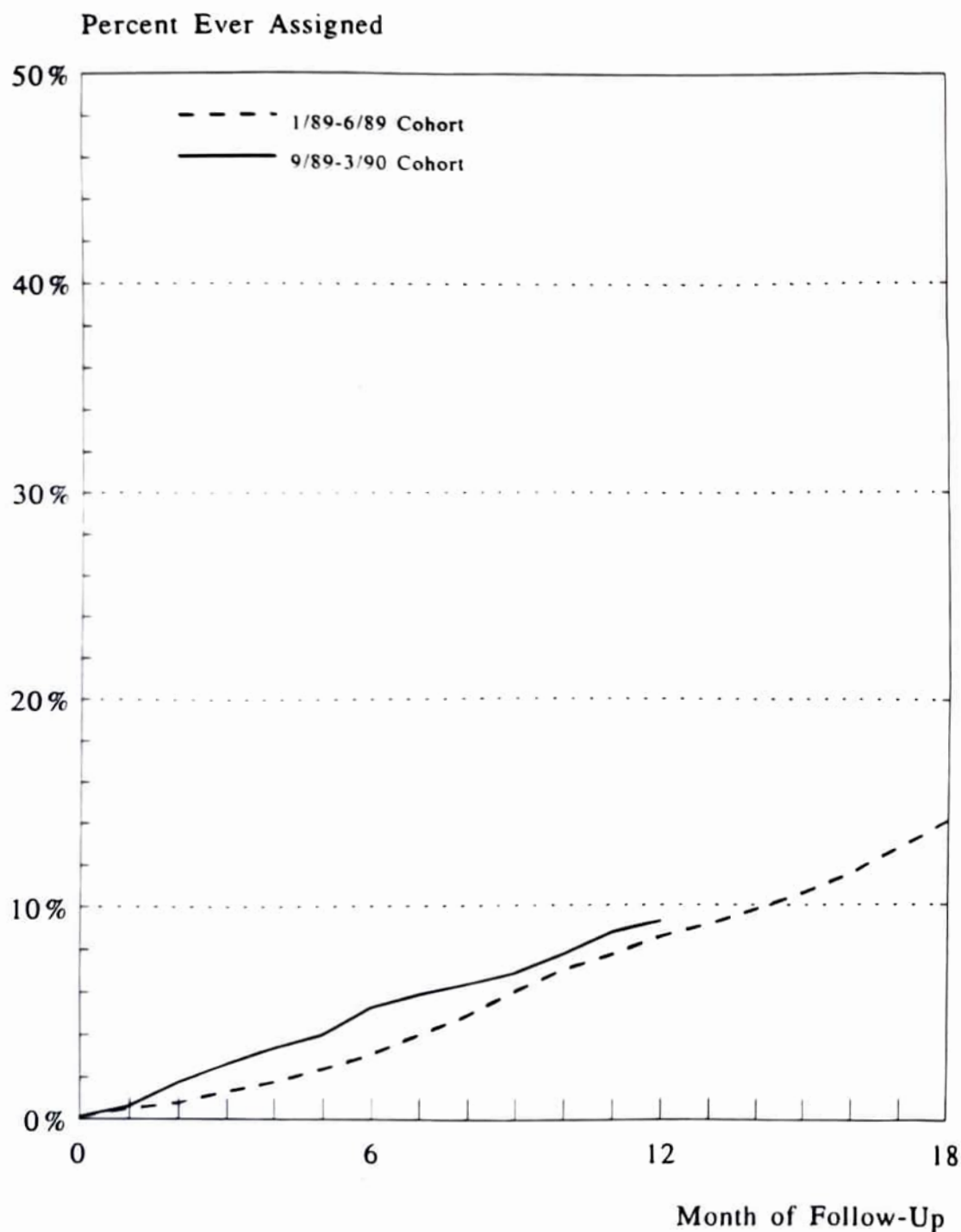


Exhibit 2.7

**Patterns of Assignment to Employment and Training Activities
for Clients in the Work Choice Treatment Group**

Activity	Percent of <u>All</u> Clients Ever Assigned to Activity ^{a, b}	Percent of <u>Ever</u> <u>Assigned</u> Clients Assigned to Activity ^a
Early Enrollees		
Basic Education	4.8	34.2
Post-Secondary Education	4.7	33.4
Job Skills Training	2.5	17.7
Job Readiness Training	2.3	16.0
Job Club	1.7	11.7
CWEP	0.2	1.4
Any Activity	14.1 ^c	100.0 ^c
Number of Clients Observed	2,601	368
Later Enrollees		
Basic Education	3.3	34.2
Post-Secondary Education	4.1	43.4
Job Skills Training	2.4	25.0
Job Readiness Training	0.6	6.6
Job Club	0.5	5.3
CWEP	0.0	0.0
Any Activity	9.5 ^c	100.0 ^c
Number of Clients Observed	799	76

^a Further explanation of how rates were calculated is provided in notes to Exhibit ES.1.

^b These percentages decline across cohorts in part because follow-up periods become shorter.

^c Sums for activities do not equal totals for Any Activity because some clients had multiple assignments.

club, and especially CWEP -- accounted for a much smaller fraction of assignments in Work Choice than under JOBS. For example, CWEP accounted for only one percent of assignments in the early Work Choice cohort, compared with fully 47 percent of assignments in the early JOBS cohort (see Exhibit 2.3). The most popular educational activities for Work Choice clients also were those which were the most time-intensive. Basic and post-secondary education accounted for 34 and 33 percent of assignments, respectively, among Work Choice volunteers in the early cohort, compared with 30 and 19 percent among early JOBS assignees.

One likely implication of the emphasis on intensive educational activities is that -- to the degree that impacts are dependent on employment and training -- the wait to program impacts can be expected to be relatively long for Work Choice. Given the limited time horizon over which outcomes were measured (at most 18 months for early entrants), it therefore seems likely that such effects would not be captured in the present analysis. Of course, other components of the Work Choice approach -- mandatory assessment and counseling, and extended transitional Medicaid and child care assistance -- could have earlier impacts that would be detectable here.

Use of Sanctions in Work Choice

The only mandatory component of Work Choice was attendance at an assessment and counseling session. However, although enrollees could be sanctioned for failure to participate in mandatory assessment and counseling, it appears that sanctioning was not used in practice. Only two participants had recorded sanctions over the demonstration period.

Summary

The Work Choice population's characteristics and employment and training participation were quite different from those of the mandatory JOBS population. It is likely that differences in the eligibility criteria, program requirements, and geography account for many of the differences.

Clients eligible for Work Choice -- which operated only in Montgomery, a large urban county -- were those from single-parent families whose youngest child was aged one through five. Because they had young children, Work Choice eligibles tended to be younger than JOBS-

mandatory clients. Higher proportions of Work Choice sample members also were non-white and in the midst of long-term welfare spells.

We measured the extent and nature of employment and training activities for which Work Choice-eligible clients volunteered. The results suggested important differences from employment and training participation for JOBS-mandatory clients. As could be expected, the proportions of clients ever volunteering for such services under Work Choice were lower than those for mandatory clients at similar intervals of follow-up. Nonetheless, the percentage of Work Choice eligibles assigned to activities -- about 14 percent after 18 months of follow-up -- was quite respectable for a voluntary program.

The intensive educational emphasis in Work Choice employment and training assignments was even greater than for JOBS-mandatory clients. Particularly striking was the nearly complete absence of CWEP placements among Work Choice assignments. This finding reinforces the general sense detected in our process study that CWEP was perceived more often to be an obligation (e.g., "working off your grant") than an opportunity.

Other things being equal, investments in intensive educational programs are expected to take longer to generate increases in employment and earnings than activities -- such as job search and job readiness training -- with a more immediate employment focus. Given the short time horizon over which Work Choice participants could be observed (at most 18 months) we do not expect to see strong positive impacts arising from employment and training participation.

It is, however, possible that the relatively few assignments to job search and job readiness had some effects during the time period covered by this study. Other Work Choice elements - - assessment and counseling, and the offer of expanded transition benefits -- also might produce positive short-term impacts on employment and earnings. In Chapter 4, we will present results from the Work Choice impact analyses in detail. We turn first to Chapter 3, which presents impact findings for JOBS.

CHAPTER THREE

JOBS IMPACT FINDINGS

JOBS might increase welfare clients' economic self sufficiency in several ways. The most basic is through clients' utilization of services designed to increase employability and help participants find jobs. Although it is a single program, JOBS provides a broad range of services, including education, job training, and job search. Some of these services can be expected to have effects in the short-run (e.g., job search assistance), whereas others may take longer (e.g., basic education). It is also possible that JOBS would affect some people who do not receive services. Because the program is mandatory for a large fraction of the ADC caseload, it might induce some clients averse to participation to leave ADC sooner than they otherwise would. Alternatively, it is possible that some people see JOBS as a benefit of ADC participation, in which case ADC exits might decrease, other factors held constant.¹

The findings presented in this chapter suggest that the impacts of Ohio's early JOBS program on clients' earnings and welfare receipt were quite small. Although JOBS had a small positive effect on employment, it appears not to have increased total earnings, decreased ADC receipt, or reduced total ADC payments. The overall results, however, mask stronger impacts on these measures for certain subgroups of JOBS participants. For example, one-parent families exhibited statistically significant reductions in ADC payments, whereas payments to two-parent families increased sharply.

Impacts that are modest -- or even negative -- may be unexpected, but they do not necessarily indicate that JOBS services are ineffective. In interpreting the results, it is crucial to consider a number of factors external to JOBS' basic philosophy and design which likely had a great affect on the program's capacity to generate impacts during its early years in Ohio.

¹ It is also conceivable that JOBS could affect the behavior of ADC clients *not* required to participate; i.e., those who qualify for exemptions. For example, there may be a demonstration effect: non-participants might be induced to increase work effort from observing similar behavior by members of their peer group who do participate. An experimental research design will detect impacts on both participants and non-participants, but cannot distinguish these effects.

Details on many of these factors were discussed in Chapters 1 and 2. One, undoubtedly, was the slow pace of program implementation. Relatively low percentages of clients ever were assigned during the early years of Ohio's JOBS program, and many of those assigned did not begin any activity until more than a year after demonstration enrollment. Consistent with the intent of the national JOBS legislation, Ohio assigned many clients to intensive basic educational activities which they could not be expected to finish quickly. Although we observed subjects for up to three years, the potential for impacts likely was greatly constrained by the timing and nature of assignments, the great amount of preparation necessary for many clients before they could meet GED requirements, and the shortage of jobs in a deep recession.

Unlike at least two other early state JOBS evaluations, the present study's design measures impacts for all mandatory clients, starting as soon as their counties implemented JOBS.² The design does not eliminate a portion of this early experience (as in the Florida and California evaluations, which commenced measurement a year or more after program start-up) or involve less than the full population of non-exempt clients (as in Florida, which included only new or re-opening cases, or in California, which limited measurement to clients who showed up for an initial program orientation). Hence, to a greater degree than other evaluations, the present results capture the full impacts of Ohio's early JOBS program.

We turn now to the detailed results of our analysis. The next section of this chapter describes our analytic approach. The subsequent three sections present the findings for employment and earnings, ADC participation and benefits, and subgroups. Section 3.6 provides a summary and interpretation of the findings.

3.1 ANALYTIC APPROACH

As explained in Chapter 1, the impacts of JOBS on welfare recipients' employment, earnings, ADC participation, and ADC benefit amounts were measured using a random assignment design. ADC clients who met the requirements for JOBS participation were assigned

² Some clients actually entered the demonstration under JOBS's predecessor in Ohio, Fair Work. As described in Chapter 1, Fair Work was similar to JOBS in many respects.

randomly either to a treatment group, which was subject to the JOBS requirements, or to a control group, which was excluded from JOBS but allowed to enroll in other (non-JOBS) employment and training services. Impacts are measured as the difference between mean outcomes for treatment- and control-group members.

As noted above, impacts are measured for the entire treatment group, including clients who never received JOBS assignments. The impacts so measured represent the total effectiveness of JOBS over the mandatory caseload. In interpreting the results, it must be remembered that more than half of the treatment group never received JOBS assignments during the follow-up period. To the extent that impacts are expected to result from JOBS participation, the modest participation rates constitute an important limiting factor.

Clients in the 15 demonstration counties were enrolled in the experiment between January 1989 and December 1991. Clients were observed from the month of their enrollment through December 1992, when the demonstration ended. Because subjects entered the demonstration at different points in time but ended participation in the same month, the maximum follow-up that was possible varied. Clients who enrolled by December 1989 all could be observed for three full years, whereas those enrolled in 1990 and 1991 could be observed only for two and one full year, respectively. Because both maximum available follow-up and important characteristics varied across cohorts, we present results impacts separately by cohort.³ Because it was observable for the longest time, we provide the greatest detail on impacts for the 1989 cohort.

Observation commenced in the first month ADC cases -- treatment and control -- were active and contained one or more recipients who were required to participate in JOBS (or, in the case of the control group, who would have been required had they been in the treatment group).⁴ The follow-up period for ADC participation and payments -- measured on a monthly basis -- started in the month after the month of demonstration entry. Follow-up for employment

³ As noted in Chapter 2, the most important distinguishing characteristic across cohorts was time on ADC. The 1989 and 1991 cohorts were comprised predominantly of ongoing ADC cases, whereas the 1990 cohort was made up mainly of new and re-opening cases.

⁴ Clients here were defined as required to participate if they did not have an official exemption from participation. Prior to July 1991, the State exempted one parent per case for care of children under age six. From that month on, the child care exemption was restricted to care for children under age three, consistent with the exemptions allowed under the Family Support Act.

and total earnings -- measured on a quarterly basis -- started in the quarter after entry.⁵ The unit of analysis for ADC-related outcomes is the case; for employment and earnings impacts, the recipient is the unit of analysis.

In addition to measuring overall impacts for clients in each cohort, we also estimated impacts in the 1989 cohort for subgroups of social or policy interest. Subgroups were defined on the basis of the following characteristics: whether they were ever employed in the six months before entering the demonstration; whether they were in a new, short, or long ADC spell at demonstration entry; whether they were in a single-parent (ADC-R) or two-parent (ADC-U) case; whether they had completed at least twelve years of school; and whether they were 25 years or older. We describe our approach to these subgroup analyses in more detail later in the chapter.

To reduce the effects of any chance differences between treatment- and control-group members and improve the precision of impact estimates, regression models were used to adjust the estimates measured. This procedure involves regressing each outcome measure on a dummy (0-1) variable indicating treatment-control status, plus a series of other client characteristics.⁶ The estimated coefficient on the treatment dummy measures the "pure" impact of the demonstration, and a two-tailed t-test on that coefficient indicates whether the impact is statistically significant. The tables below note which impacts are significant.

The next section presents our estimates of JOBS' impacts on employment and earnings separately for 1989 entrants and later entrants. After this, we present findings for ADC receipt and payments. The third section following analyzes impacts for subgroups. We conclude with

⁵ The quarterly basis for measurement of employment and earnings reflects the fact that Unemployment Insurance wage data collected and provided by the Ohio Bureau for Employment Services are not available for finer time units.

⁶ Characteristics were measured as of the month of demonstration entry. Other covariates included were: duration of ADC spell in months; client's age; county unemployment rate; total number of children and number less than three years of age; client's age in the case; and dummy variables (zero-one indicators) for case type (ADC-R/ADC-U), recent employment (yes/no), race (white/nonwhite), relatives receiving ADC (two dummies to capture yes/no/missing), and migration status (migrant/nonmigrant). Migration status refers to whether or not a sample member who was observed in a given time period ever subsequently migrated out of the demonstration counties. Follow-up was truncated for outmigrants starting in the time period in which they moved; that is, they were observed only up through the last period for which they were continuously present.

a comparison of the Ohio JOBS findings with impacts from evaluations of JOBS in California and Florida and a general discussion of the findings.

3.2 IMPACTS OF JOBS ON EMPLOYMENT AND EARNINGS

JOBS services are intended to increase employment and earnings by raising clients' potential labor productivity (through education and training, and CWEP), improving their job-search skills (through Job Club), or stimulating potential employers' demand via wage subsidies.⁷ Some of these activities are intended to have impacts in the short-run, especially job search, while others are likely to have positive impacts only after a year or more of participation, such as basic education.

We focus on two outcomes in this section: employment rates and average annual earnings. The employment rate is defined as the fraction of treatment- and control-group members ever employed during a particular year or quarter after entering the demonstration. Earnings are expressed as total dollar amounts for a particular follow-up year or quarter, averaged over all clients -- working and not working -- in each period.

Results for Early Demonstration Entrants

Exhibit 3.1 presents impacts on employment and earnings for clients in the 1989 cohort; that is, those who enrolled in the first year of the demonstration. Columns 1 through 3 show results for employment, and columns 4 to 6 provide the effects on average earnings. Column 1 gives the average employment rate in each year of follow-up for the control group, and Column 2 gives the impact in absolute terms; i.e., the difference between the average employment rates for treatment- and control-group members. Column 3 divides the absolute impact by the control group mean to get the proportionate impact, which gives a useful basis for

⁷ Although Ohio's JOBS plan had a subsidized employment component, only a few clients in the demonstration counties received this assignment.

Exhibit 3.1

Impacts of JOBS on Employment and Total Earnings for
Early (1989) Demonstration Entrants^a

Follow-Up Interval	Percent Ever Employed			Average Total Earnings		
	Control Group (1)	Impact ^b (2)	Relative Impact ^c (3)	Control Group (4)	Impact ^b (5)	Relative Impact ^c (6)
Year 1	36.9	3.7***	10.0	\$1,860	-\$61	-3.3
Year 2	41.3	2.9**	7.0	2,563	76	3.0
Year 3	42.2	3.3**	7.8	3,129	23	0.7
Years 1-3	56.5	4.3***	7.6	7,552	38	0.5

^a Based on a total of 1,314 control and 7,860 treatment group members. Numbers for Years 2 and 3 decline somewhat due to outmigration from the demonstration counties of some sample members.

^b The impact is the difference between the treatment-group mean (not shown) and the control group mean (shown in columns 1 and 4).

^c The relative impact is the impact expressed as a percentage of the control-group mean.

* Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

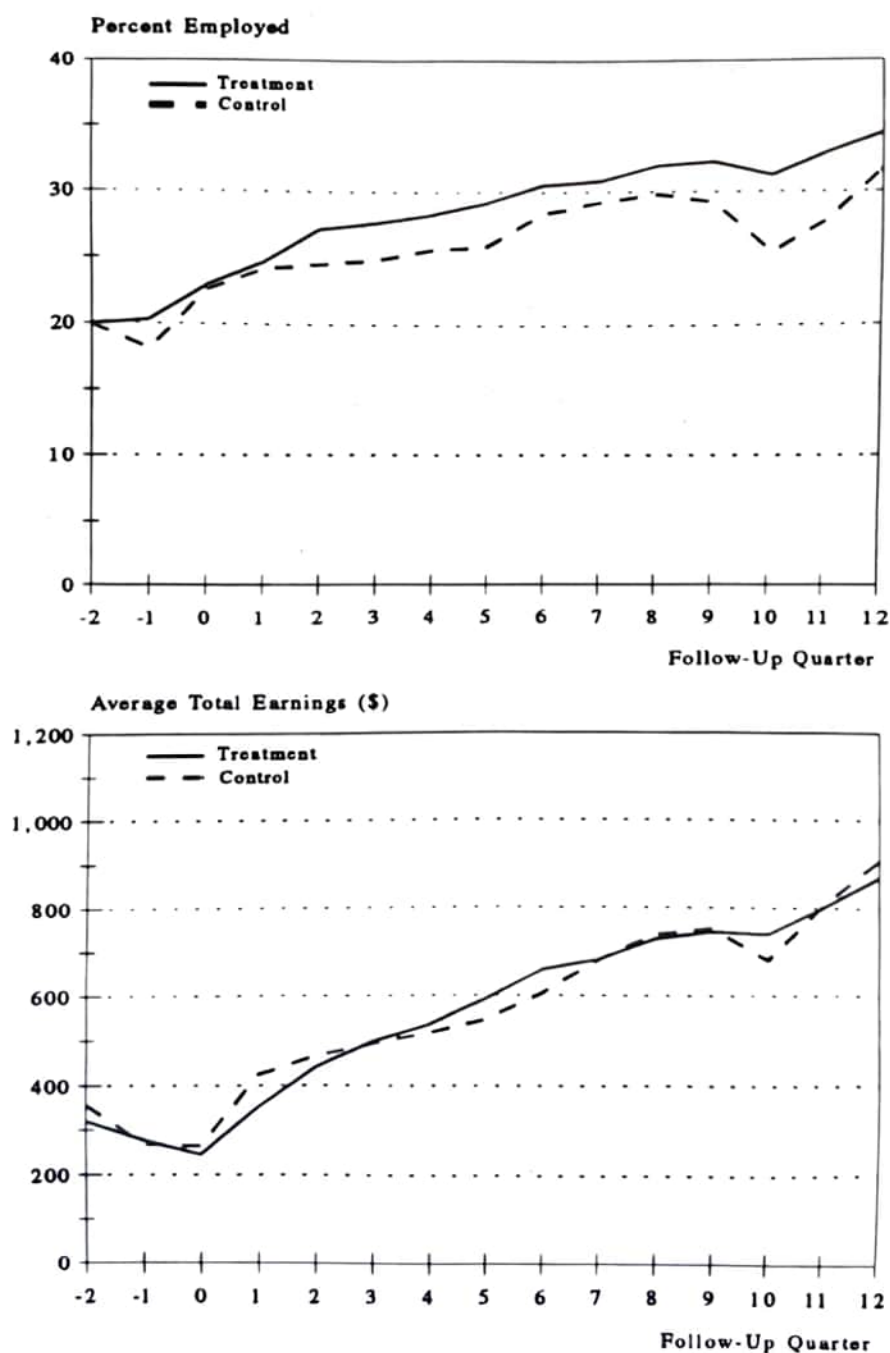
comparing impacts over time and different outcome measures. Columns 4 through 6 are the analogous measures for average earnings.

Overall, Ohio's JOBS program had small, but statistically significant positive impacts on employment rates for early demonstration entrants, and virtually no impact on earnings. Over the entire three-year follow-up period, 60.8 percent of the treatment group and 56.5 percent of the control group were ever employed: a proportionate difference of 7.6 percent. The exhibit shows that employment rates for the treatment group were higher in each of the three years following entry. The three-year impact on average total earnings was small -- only \$35 -- and statistically insignificant.

The graphs in Exhibit 3.2 provide more detail on the time paths for employment and earnings impacts. Employment impacts were fairly consistent in size: the top graph reveals that, for 1989 entrants, the treatment group's average employment rate was higher than the control group's for every follow-up quarter from 1 through 12. The impacts peaked in quarter 10, coincident with a drop in the control group's employment rates. Quarterly earnings impacts, shown in the bottom graph, switched between positive and negative but were consistently small. The graphs also show that both experimental and control groups experienced a steady increase in employment and earnings over the three-year follow-up period.

The finding that estimated proportionate employment impacts are larger than earnings impacts has at least two plausible explanations. One is that although JOBS did place more clients in jobs than would have worked otherwise, the additional clients worked at lower wages, fewer hours, or for shorter time periods in these jobs than average workers. Another is that by encouraging some clients to *defer* or *reduce* employment in order to participate in employment and training, JOBS may have diminished work effort by a number of clients whose earnings potential was high relative to that of clients who the program helped to find jobs. This kind of effect could tilt the balance of employed clients towards those with relatively low earnings and, conceivably, could even explain negative impacts on total earnings found among some subgroups (see Section 3.4).

Exhibit 3.2
Employment and Average Total Earnings
by Follow-Up Quarter:
Early (1989) JOBS Demonstration Entrants



Notes: Quarter 0 is the quarter of demonstration entry.
Quarters -1 and -2 are the two quarters preceding entry.

Results for Later Demonstration Entrants

This section looks at employment and earnings impacts for clients in the later cohorts: those who enrolled in the Ohio JOBS demonstration during 1990 and 1991. An important distinguishing characteristic of 1990 entrants is that they were much more likely to be in new or re-opened ADC cases, as most ongoing cases entered the demonstration in 1989. In contrast, a majority of 1991 cohort members also were from ongoing cases, due to the large influx of single-parent demonstration entrants from Franklin County in that year (see Chapter 2).

The top half of Exhibit 3.3 presents impacts for the 1990 and 1991 cohorts. Columns 1 through 3 show impacts for 1990 entrants, and the last three columns give results for 1991 entrants. As in Exhibit 3.1, the columns give the control group means and the absolute and proportionate impacts. Impacts could be observed for up to two years of follow-up for 1990 entrants but only one year for 1991 entrants.

The most striking aspect of findings for later cohorts is the evidence of negative employment and earnings impacts for 1990 demonstration entrants. Proportionate impacts on average total earnings were especially noticeable: mean earnings for the treatment group were 14 percent lower than earnings for the control group in the second year of follow-up, representing an absolute impact of -\$482.

As noted above, the most striking difference between 1989 and 1990 entrants is that a minority of the former and a majority of the latter were from new or re-opening cases. Later, we will see that subgroup analyses found that new and re-opening cases in the 1989 cohort also showed negative total earnings impacts. This strong consistency both affirms the likelihood that the negative finding is not a chance occurrence and spurs thinking about why JOBS might cause negative impacts among a relatively less welfare-dependent population.

One possibility is that the opportunity costs associated with participating in similar activities vary by cohort. It is likely that clients in new or re-opened cases have, on average, more recent work experience and greater labor force attachment, and are more likely to return to work on their own than clients from ongoing cases. If JOBS assignments defer the return to work for a portion of such clients, the results could be expressed in negative impacts for cohorts

Exhibit 3.3

Impacts of JOBS for Later (1990, 1991)
Demonstration Entrants

Outcome and Follow-Up Interval	1990 Entrants ^a			1991 Entrants ^b		
	Control Group (1)	Impact ^c (2)	Relative Impact ^d (3)	Control Group (4)	Impact ^c (5)	Relative Impact ^d (6)
Percent Ever Employed						
Year 1	47.5	-1.4	-3.1	43.3	2.1**	4.8
Year 2	49.1	-3.3*	-6.7	--	--	--
Average Total Earnings						
Year 1	\$2,322	-\$319**	-13.7	\$2,066	-\$95	-4.6
Year 2	3,421	-482**	-14.1	--	--	--
Percent of Months Received ADC						
Year 1	72.1	-0.6	-0.8	81.1	-0.5	-0.6
Year 2	47.2	0.7	1.5	--	--	--
Average Total ADC Payments						
Year 1	\$2,797	-\$2	-0.1	\$3,198	-\$2	-0.1
Year 2	1,857	88	4.7	--	--	--

^a Employment and earnings results are based on a total of 750 control and 5,239 treatment group members. Results for ADC outcomes are based on a total of 616 control and 3,622 treatment group cases. Numbers for Year 2 decline somewhat due to outmigration from the demonstration counties of some sample members.

^b Employment and earnings results are based on a total of 2,307 control and 11,021 treatment group members. Results for ADC outcomes are based on 2,197 control and 9,529 treatment group cases.

^c The impact is the difference between the treatment group-group mean (not shown) and the control group mean (shown in columns 1 and 4).

^d The relative impact is the impact expressed as a percentage of the control-group mean.

* Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

containing high proportions of new and re-opened cases. Further evidence supporting this second interpretation will be discussed in reviewing findings for other subgroups.

Another explanation might be that new cases received different JOBS services, services which brought particularly heavy "opportunity costs." Patterns of employment and training participation examined in Chapter 2 suggest a slight tilt towards more intensive assignments for 1990 entrants, compared with 1989 entrants. For example, Exhibit 2.3 shows that 33 percent of ever assigned 1990 entrants were assigned to basic education, compared with 30 percent of 1989 entrants. The corresponding fractions for post-secondary assignments -- another intensive category -- were 24 percent and 19 percent, respectively. However, Exhibit 2.4 in Chapter 2 reveals no similar differences in assignments for new compared with existing cases among 1989 entrants assigned to employment and training activities.

Opportunity costs for 1990 cohort members also may have been greater because this cohort tended to receive assignments earlier -- and hence spend more time in them -- than members of either 1989 or 1991 cohorts. Statistics for activity durations in Fein (1994, Exhibit 9) tend to support this hypothesis. For example, by the end of their first year of follow-up, those who had been assigned in the 1990 cohort averaged 1.7 months longer in assignments than assignees in the 1989 cohort.

3.3 IMPACTS OF JOBS ON ADC RECEIPT AND PAYMENTS

We expect JOBS' effects on ADC receipt to occur primarily as a by-product of program impacts on earnings, and typically to be in the opposite direction. The modest earnings impacts just reviewed, and constraints on the elasticity of the earnings-ADC payment relationship cause us to have modest expectations for impacts on ADC outcomes.⁸

⁸ There are two reasons why a given proportionate impact on earnings is expected to have a smaller proportionate effect on ADC receipt, all things being equal. One is that ADC grant formulas disregard a small amount of earnings for the first four months of work and make larger allowances for child care and work expenses (which continue longer than four months); thus, a client could potentially increase her earnings by several hundred dollars a month without triggering a reduction in her ADC grant. Another reason is that any fraction of a large earnings gain that exceeds the ADC eligibility threshold can, by definition, have no further effect on ADC payments.

In this section, we present impacts for two ADC outcomes: ADC participation rates, and ADC payments. ADC participation is measured at the case level as the average percentage of months in a specified follow-up period -- e.g., a year or quarter -- that subjects were receiving assistance. For example, if on average cases are active on ADC for nine of the first twelve months after entering the demonstration, then the ADC participation rate is .75 for the first year of follow-up. The second outcome, ADC payments, is measured in terms of average annual payments and includes months with zero payments for clients leaving ADC.

Results for Early Demonstration Entrants

Exhibit 3.4 presents the impacts of JOBS on ADC receipt and payments for 1989 demonstration entrants. Over the three years of follow-up, ADC receipt and average total payments to the control group (columns 1 and 4) decline markedly, reflecting the well-known tendency for many welfare clients to use ADC on a short-term basis. Columns 2 and 5 show that JOBS had no statistically significant impacts on ADC receipt or payments, respectively. This conclusion is reinforced by the graphs in Exhibit 3.5, showing the path of ADC impacts over the follow-up period. Rates of ADC receipt and average total payments for treatment and control groups track each other very closely over the entire period.

Results for Later Demonstration Entrants

The ADC impacts for later entrants are contained in the bottom two panels of Exhibit 3.3. As for the 1989 cohort, JOBS had no statistically significant effects on ADC receipt or payments for either the 1990 or 1991 cohorts.

The actual magnitudes of impacts for ADC receipt and payment are generally smaller than the corresponding impacts on average total earnings. This is especially noticeable for the 1990 cohort, which shows little impact on ADC payments but average earnings that are substantially lower for the treatment group than for the control group.

The earnings impacts represent an average effect calculated across all members of each demonstration cohort. Lower average total earnings for treatment-group members could reflect

Exhibit 3.4**Impacts of JOBS on ADC Receipt and Total ADC Payments
Early (1989) Demonstration Entrants^a**

Follow-Up Interval	Percent of Months Received ADC			Average Total ADC Payments		
	Control Group (1)	Impact ^b (2)	Relative Impact ^c (3)	Control Group (4)	Impact (5)	Relative Impact ^c (6)
Year 1	80.7	-0.8	-1.0	\$3,092	-\$30	-1.0
Year 2	60.5	0.1	0.2	2,366	-13	-0.5
Year 3	46.7	2.1	4.5	1,845	91	4.9
Years 1-3	63.9	0.1	0.2	7,303	51	0.7

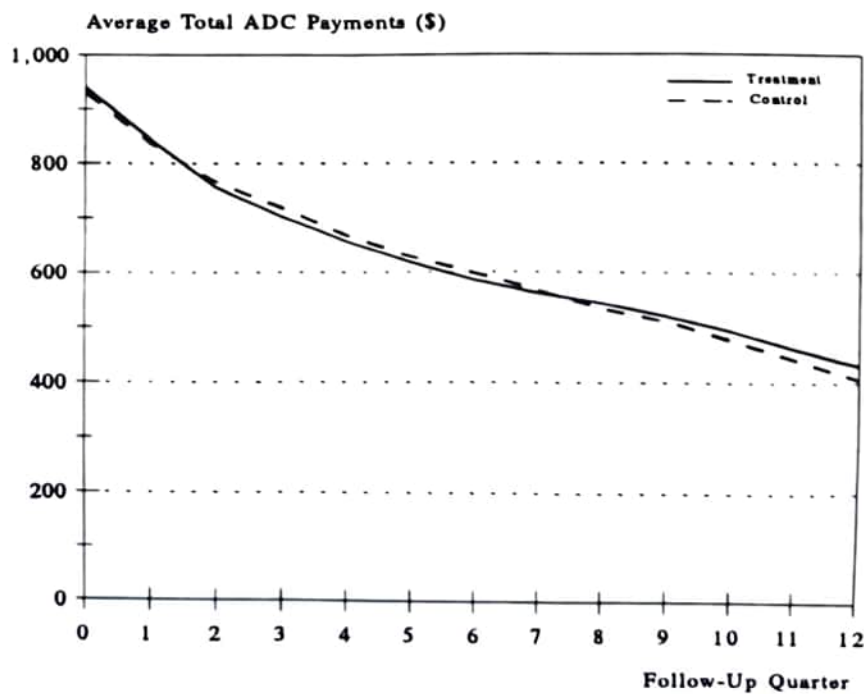
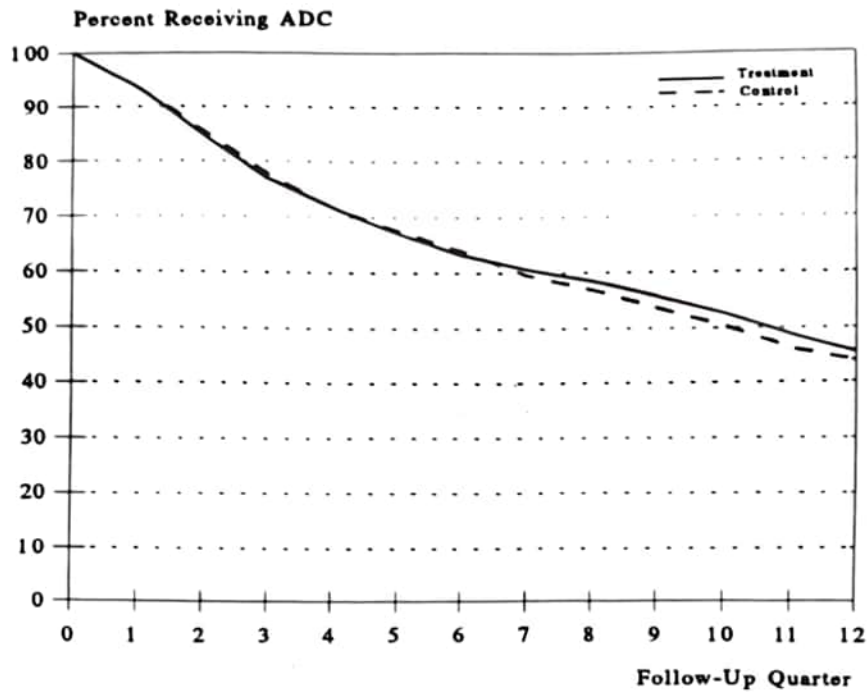
^a Based on a total of 1,240 control and 6,807 treatment group cases. Numbers for Years 2 and 3 decline somewhat due to outmigration from the demonstration counties of some sample members.

^b The impact is the difference between the treatment-group mean (not shown) and the control group mean (shown in columns 1 and 4).

^c The relative impact is the impact expressed as a percentage of the control-group mean.

* Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

Exhibit 3.5
ADC Receipt and Average Total Payments
by Follow-Up Quarter:
Early (1989) JOBS Demonstration Entrants



Note: Quarter 0 is the quarter of demonstration entry

either a large gap for a relatively small group of clients or smaller and more dispersed deficits spread over a wider group of clients. To the extent that effects were larger and more concentrated, they could be expected to be accompanied by significant increases in ADC payments. The fact that they were not suggests that the earnings impacts were dispersed, and too small to substantially offset the earnings and child care disregards.

3.4 IMPACTS OF JOBS FOR KEY CLIENT SUBGROUPS

It is possible that even though overall impacts were modest, impacts were more positive for some segments of the JOBS-mandatory ADC caseload than for others. In general, such variation could arise from differences in either the nature of JOBS services provided or differences in clients' propensities to respond to a given service. Exploring subgroup impacts can yield valuable insights into the sources of the overall impacts.

Subgroups were chosen for this analysis on the basis of their social or policy significance. Criteria defining subgroups included whether, at the time of demonstration enrollment, clients:

- Ever were employed in the six months before entering the demonstration;
- Had just started ADC, had been receiving ADC in their current spell for under two years, or had been receiving ADC for two years or more;
- Had completed at least twelve years of school;
- Were in a single-parent (ADC-R) or two-parent (ADC-U) case; and
- Were less than 25 years old.

The first four characteristics on the list measure a number of general dimensions of labor force attachment and socioeconomic disadvantagedness.⁹ Prior studies have found that

⁹ Family type (single- versus two-parent) is a proxy for work experience, given that unemployed parent rules require work history as a condition of eligibility. Of course, family type also embraces additional dimensions of possible concern. Notably, single parenthood brings a myriad of other employability problems besides low work experience, such as special child care needs.

employment and training programs often have the strongest impacts for moderately disadvantaged clients (Friedlander 1993). The most commonly offered explanation is that recipients with greater labor force attachment and higher skill levels have less need for the new services, and those with lower levels of education and experience are difficult to help. The last characteristic is of interest in view of recent national proposals which would implement new welfare reform policies for younger recipients first.

We present subgroup impacts for the 1989 cohort only, to simplify the presentation and because that cohort was observed for the longest follow-up period. We used F-tests to determine whether variation in impacts across subgroups for a given dimension was statistically significant and present detailed estimates in tabular form only for subgroups where there was significant variation for impacts on either average total earnings or average ADC payments. Whereas F-tests indicate whether subgroup impacts are statistically different across subgroups, t-test results in the exhibit indicate whether impacts for individual subgroups differed from zero statistically (i.e., whether or not there was an impact). Findings for subgroup-defining dimensions which did not show statistically significant differences are described in a separate section below.

The results reveal that there was important variation in program impacts across subgroups. In broad terms, the subgroup findings suggest that JOBS was most advantageous for clients who were relatively job-ready but not yet strongly attached to the labor force (for example, high school graduates with no earnings in the two quarters preceding demonstration entry). Impacts tended to be negligible, or even negative, for clients who already had relatively high labor force attachment prior to JOBS (e.g., those with recent work experience). Poorer results also appeared for the least educated clients (e.g., those with less than 12 years of school), compared with those who had greater education.

We turn now to the detailed results of these analyses. After presenting the basic subgroup findings, we consider the extent to which differences in employment and training services provided to different kinds of clients might explain the findings.

Recent Work Experience

We defined clients as having recent work experience if they were ever employed in the six months before enrolling in the demonstration. This characteristic is a measure of labor force attachment. Comparing clients with and without recent work experience, other research has shown that welfare-to-work programs tend to produce larger benefits for those with work experience.¹⁰ In a welfare-to-work program in New York State, Hamilton *et al.* (1993) found larger relative earnings gains for those with no earnings at enrollment. Based on a review of four experimental evaluations of welfare-to-work programs, Friedlander (1993) found more positive impacts when clients with \$3,000 or more in prior earnings were dropped from the samples. Clients who are relatively attached to the labor force -- such as those with recent work experience -- may have more to lose from participating in JOBS, at least in the short run, to the extent that they postpone their paid employment to meet the participation requirement.

Exhibit 3.6 presents the findings for work experience and two other subgroup categories. To simplify the analysis, we consider only the two most broadly relevant outcomes: average total earnings and average total ADC payments. The table also presents sample sizes for the treatment and control groups in each subgroup. The widely varying sample sizes serve as a reminder that differences in statistical significance for impacts may often reflect variation in sample size rather than actual differences in the presence of effects.

Ohio's JOBS program was more beneficial for clients with no recent work experience than for those who recently had worked. The treatment-control difference in average earnings is positive for the former group and negative for the latter. Although impacts for individual subgroup are generally not statistically significant, the differences in earnings between the two subgroups in the first year of follow-up, and for all three years combined (not shown), are

¹⁰ Even though *impacts* may be more favorable for clients with no recent work experience, these clients tend to be more dependent on welfare than those *with* work experience, according to other research.

Exhibit 3.6

**Impacts of JOBS on Total Earnings and Total ADC Payments for Key Subgroups:
Early (1989) Demonstration Entrants**

Subgroup and Follow-Up Interval	Average Total Earnings				Average Total ADC Payments					
	Control Group (1)	Impact ^a (2)	Relative Impact ^b (3)	Sample Size Control (4) Treatment (5)	Control Group (6)	Impact ^a (7)	Relative Impact ^b (8)	Control (9)	Treatment (10)	
<u>Ever Employed in Six Months Before Demonstration Enrollment?</u>										
No										
Year 1	\$902	\$111	12.3	834	4,738	\$3,284	-\$35	-1.1	778	4,013
Year 2	1,633	199	12.2			2,538	-29	-1.1		
Year 3	2,348	91	3.9			2,017	53	2.6		
Yes										
Year 1	4,247	-351**	-8.3	480	3,122	2,615	-21	-0.8	462	2,794
Year 2	4,948	-134	-2.7			1,937	15	0.8		
Year 3	5,195	-101	-1.9			1,425	157*	11.0		
<u>Duration of ADC Receipt in Current Spell</u>										
New/Re-Opened Case										
Year 1	\$3,085	-\$418**	-13.5	418	2,629	\$2,639	-\$155*	-5.9	401	2,293
Year 2	4,009	-403	-10.1			1,779	-35	-2.0		
Year 3	4,998	-776**	-15.5			1,267	71	5.6		
Less Than Two Years										
Year 1	1,478	292	19.8	275	1,752	3,049	-24	-0.8	257	1,509
Year 2	2,374	457	19.3			2,196	38	1.7		
Year 3	2,934	358	12.2			1,696	169	10.0		
Two Years or More										
Year 1	1,160	10	0.9	621	3,479	3,460	54	1.6	582	3,005
Year 2	1,619	218	13.5			2,883	-23	-0.8		
Year 3	1,920	389	20.3			2,337	66	2.8		

(continued on next page)

Exhibit 3.6
(continued)

Subgroup and Follow-Up Interval	Average Total Earnings				Average Total ADC Payments			
	Control		Treatment		Control		Treatment	
	Group (1)	Impact ^a (2)	Relative Impact ^b (3)	Sample Size (4)	Group (6)	Impact ^a (7)	Relative Impact ^b (8)	Sample Size (9)
Family Type								
Single-Parent								
Year 1	\$1,521	\$19	1.2	917	\$3,016	-\$116**	-3.8	910
Year 2	2,177	115	5.3	4,366	2,458	-197***	-8.0	4,187
Year 3	2,482	303	12.2		1,952	-103	-5.3	
Two-Parent								
Year 1	2,311	-172	-7.4	397	3,212	117*	3.6	330
Year 2	3,073	19	0.6	3,494	2,193	313***	14.3	2,620
Year 3	4,038	-395	-9.8		1,640	443***	27.0	

Note: F-tests were performed to detect differences in impacts among categories within each subgroup for each year of follow-up. The difference in impacts on average total earnings across subgroup categories is statistically significant for the following: (1) Ever Employed in Six Months Before Demonstration Enrollment: Year 1 differences significant at the 1% level; (2) Duration of ADC Receipt in Current Spell: Year 1, 5% level; and (3) Family Type: Year 3, 5% level. Analogously, the difference in impacts on average total ADC payments across subgroup categories is statistically significant for the following subgroups and years: (1) Duration of ADC Receipt in Current Spell: Year 1, 10% level; and (2) Family Type: all three years, 1% level.

^a The impact is the difference between the treatment-group mean (not shown) and the control-group mean (shown in columns 1 and 6).

^b The relative impact is the impact expressed as a percentage of the control-group mean.

* Impacts statistically different from zero at the 10% level; ** at the 5% level; *** at the 1% level.

significant.¹¹ Note that clients with recent work experience in the control group had much higher average earnings in each year of follow-up than control-group members with no recent work experience, reinforcing the interpretation that this dimension provides a strong proxy for labor force attachment.

Impacts on ADC payments for both subgroups tend to be substantially smaller than, and opposite in direction from, the earnings effects. This is not surprising given the earnings offset in the ADC program. None of the individual ADC impacts or subgroup differences is significant, however.

The weaker results for the subgroup with recent work experience are consistent with the view that JOBS was less beneficial to those with relatively strong labor force attachment. A possible explanation for this is that those who are relatively attached to the labor force need to reduce that connection somewhat in order to participate in JOBS. This explanation is supported by the fact that the largest negative earnings impact occurred in year 1, when participants may have been more likely to postpone their employment to participate in JOBS.

Duration of Current ADC Spell

The length of time recipients had been continuously receiving ADC as of the month they entered the demonstration provided an indicator for degree of welfare dependence. In the subgroup analyses, clients were distinguished according to whether they were just beginning a welfare spell at demonstration entry, in a spell for less than two years, or in a spell of two years or more. That this is a reasonably good proxy for welfare dependency can be seen by comparing mean earnings and ADC payments for control-group members across the three

¹¹ Two different tests are used in evaluating differences in impacts across subgroups, and the tests sometimes lead to this apparent inconsistency. One test (F-test) provides a gauge for whether *differences in impacts across subgroup categories* are greater than those that would be expected by chance. Having established significant variation, one seeks to determine *which of the individual subgroup impacts differ from zero* to an extent unlikely to occur by chance.

subgroups. As shown in Exhibit 3.6, new cases have the highest earnings in each year, and long-term cases the lowest earnings.¹²

As noted earlier, previous research on welfare-to-work programs has generally found the largest welfare savings for the most disadvantaged recipients, and the largest earnings gains for moderately disadvantaged clients. Based on this, we might expect to see the least positive impacts for new clients beginning an ADC spell, because these will tend to be the least disadvantaged. Further, the short-term ADC spell subgroup may be considered moderately disadvantaged, and so we might expect the biggest earnings gains for this subgroup.

As shown in Exhibit 3.6, the findings generally accord with these expectations. Averaging across the three follow-up years, earnings impacts are largest (although not large enough to differ from zero statistically) for the short-term spell subgroup. The new spell subgroup has the poorest impacts, and these are significant for two of three years. Despite the negative impacts, the average earnings for treatment-group members in this subgroup still grow by nearly 30 percent per year between the first and third years of follow-up, a marked improvement in absolute terms. The differences in average earnings across the three subgroups are significant in year 1.

As with the work experience subgroups, the ADC impacts are smaller than the earnings effects. Only one of the nine yearly impacts on ADC payments is significant. There is no clear relationship between the directions of the effects on ADC payments and earnings.

The results for these subgroups are again consistent with the view that JOBS provided the least benefits to those with relatively strong labor force attachment, supporting the hypothesis advanced at the beginning of this section that JOBS was most advantageous for those who were relatively job-ready but not strongly attached to the labor force. The variation in impacts among categories of entrants again demonstrates that the weak overall impacts mask positive effects on a significant segment of entrants.

¹² Being new to welfare only indicates that clients have relatively low welfare dependence on average. Although the typical new client tends to leave welfare much faster than existing clients, some of the former will remain on the rolls for a long time.

One- versus Two-Parent Families

The differing circumstances of one- and two-parent ADC cases make it plausible that their responses to the JOBS program would be dissimilar, but it is not clear in what way. Two-parent cases tend to be more attached to the labor market and therefore to have higher incomes, which can be seen in Exhibit 3.6 in the average earnings for control-group members. Two-parent cases also may be less constrained by the need for child care because of the presence of two adults in the household, lowering a major barrier to work. The limited research evidence on the effects of welfare-to-work programs on two-parent families has generally found smaller impacts on employment and earnings, and similar effects on AFDC receipt compared to one-parent families (Gueron and Pauly 1991; Friedlander 1993).

Exhibit 3.6 shows that single-parent cases fared substantially better under JOBS than two-parent cases. ADC-Rs had positive -- although not significant -- impacts on earnings in every year, while ADC-Us had negative impacts in two of the three years, although these were also not significant. The difference in earnings between the two subgroups is significant in the third year of follow-up.

In contrast to the previous subgroup categories, the impacts on ADC payments for the family-type subgroups were somewhat larger and more significant than the effects on earnings. ADC payments impacts were in the opposite direction from earnings impacts, and five of the six yearly ADC payment impacts for the two subgroups were statistically significant. Average ADC payments to one-parent families in the treatment group were four to eight percent below the corresponding control group values. On the other hand, average payments were substantially larger to treatment- than to control-group members for two-parent families, reaching a 27-percent impact by year three. The size of this increase is surprising. Given the shorter stays on ADC for most two-parent households, it appears that participating in JOBS causes them to stay on ADC longer than they otherwise would.

These findings are again consistent with the generalization that JOBS was less beneficial to those who were relatively attached to the labor force (in this case ADC-Us). The positive effects on single-parent cases are difficult to attribute to a particular characteristic of this

subgroup, because the subgroup includes families with a wide variety of characteristics. As a hypothetical example, it is possible that the positive impacts for single-parent families are concentrated among those with 12 years of schooling.

Possible Explanations for Subgroup Differences

Two possible sources of subgroup variation in impacts are differences in program services provided to different subgroups and differences in how clients in each subgroup respond to similar services. We already have noted that negative impacts on earnings tended to be concentrated in subgroups (and cohorts) with greater labor force attachment and have suggested that this may be because such clients incur greater opportunity costs when participating in JOBS. However, it is also possible that differences in JOBS services explain some of the variation.

We can look for evidence on the latter hypothesis by examining how third-year subgroup impacts varied with rates of assignment to different activities.¹³ Given the small number and overlapping nature of subgroups, conclusions based on such analysis remain highly tentative.

Of the different employment and training assignments, post-secondary education was the only assignment whose rate was associated positively with favorable outcomes (positive earnings impacts and negative ADC payment impacts). The association was a strong one, however, implying that variation in post-secondary education assignment rates also may be an important source of observed differences in impacts across subgroups.

Basic education had little association with impacts when post-secondary education assignments were held constant. Similarly, job search, job readiness, and CWEP assignment rates did not correlate positively with favorable outcomes. In the case of job club and job readiness, this finding cannot be taken as evidence of ineffectiveness, since variation in assignment rates for these activities was too small to explain subgroup variation in impacts.¹⁴

¹³ A given activity's assignment rate for the entire subgroup indicates the potential for impacts better than its proportion among ever-assigned clients, since we are trying to explain impacts on the entire subgroup. The proportionate distributions in Exhibit 2.4 are useful mainly in comparing the relative emphasis on different assignments across subgroups.

¹⁴ The combined assignment rate for job club and job readiness only varied from 12 to 15 percent across subgroups. A non-experimental analysis of the effects of assignments to different activities found that job club and job readiness assignments were associated with positive impacts on employment and earnings (Fein, forthcoming). The non-experimental analysis also confirmed that of the remaining activities, post-secondary education was the only assignment associated with positive impacts.

Other Subgroups

We also investigated impacts for subgroups distinguished in terms of years of education and age of the case head. The results are not presented in tabular form because F-tests found no statistically significant differences in impacts across these variables.

We classified clients by education according to whether they possessed less than 12 years of education, or at least 12 years at the time of demonstration enrollment. This classification fits the general rule used by caseworkers to distinguish job-ready clients: those with 12 years of school are much more likely to be assigned to job training or job search and much less likely to be assigned to basic education. Among members of the control group, employment rates were about 20 percent higher for clients with than for those without 12 or more years of education, and average earnings were roughly 50 percent larger.

Although differences in earnings impacts by education were not statistically significant, we feel that they are worth reporting because additional tests showed them to be consistent with differences in employment impacts which were significant.¹⁵ JOBS had a statistically significant 11 percent impact on third-year earnings among clients with 12 or more years of education, but only a small and insignificant impact for clients with less than 12 years of education.

The more positive findings for clients with greater education accord with those from other JOBS evaluations (Friedlander *et al.* 1993; Kemple and Haimson 1994). That the impacts were not stronger might be due to this subgroup's somewhat stronger attachment to the labor force, a characteristic we believe to be associated with weaker impacts.

The final subgroup classification we examined distinguished clients according to whether the age of the case head of the case was less than 25 years, or 25 and above. This distinction is relevant to welfare reform proposals, which would phase in reforms beginning with young clients. From a policy standpoint, this raises the question of whether younger clients are more likely to benefit from employment and training programs. We found no clear differences in

¹⁵ The greater variance in earnings than employment means that proportionate impacts must be greater for the former than for the latter before they will be statistically significant.

impacts between the two subgroups. Neither subgroup had significant impacts on earnings, ADC payments, or ADC receipt, and both subgroups had significant positive impacts on employment. These were somewhat larger for the 25-and-older group, but the differences were not significant.

Impacts by County

A number of factors that might influence the size of impacts are likely to vary by county. These factors include: local employment conditions; degree of urbanization; demographic characteristics of recipients; JOBS participation patterns; and differing approaches to administering JOBS.

Because sample sizes were too small to allow separate impact estimates for every county, we estimated impacts for the five counties with the largest sample sizes, and for a residual category of the ten smaller counties. There was a substantial amount of variation in the direction and size of impacts across counties, and few of the estimates were statistically significant. Because of the imprecise nature of the county-level estimates and the potential for confusion about what was driving the differences, we present the results only in general terms.

Despite the imprecision of the estimates, the county-level results for the most part mirror findings at the aggregate level. There was no single county with extreme positive effects or negative effects. Employment impacts for early entrants were positive in three of the six county subgroups examined, and were larger than the effects on earnings in four of the six counties, as in the overall impact results. The results for 1989 entrants were more positive than those for 1990 entrants, which was also true in the aggregate. Counties with stronger positive effects on earnings tended to exhibit larger negative impacts on ADC payments.

There are many possible reasons for county differences in impacts. Our analysis uncovered no single factor of overriding importance. There was no clear relationship between impacts and JOBS participation patterns by county. There was also no strong relation between impacts and county unemployment rates. It appears, therefore, that county variations are driven by the interaction of a number of factors. This conclusion is consistent with findings from other

JOBS evaluations, which also have not fully accounted for county-level differences in impacts (Friedlander *et al.* 1993, Kemple and Haimson 1994).

3.5 COMPARISON TO FINDINGS FROM OTHER RECENT JOBS EVALUATIONS

In general, the impacts in Ohio are smaller than those for two other large-scale random assignment JOBS demonstrations, the GAIN program in California and Project Independence in Florida. Care is needed when comparing the Ohio results with findings from other JOBS demonstrations, because of the numerous differences in how the programs were implemented, and also because of differences in research design. These differences include:

- **Lower participation rates in Ohio.** One year after enrollment, only 23 percent of 1989 cohort members in Ohio had been assigned to one or more JOBS employment and training activities, compared with 43 percent in Florida and 36 to 66 percent across California demonstration sites.
- **Slower implementation.** There was a long lag in Ohio between demonstration enrollment and first assignment.
- **Greater emphasis on CWEP.** Ohio placed an unusual reliance on CWEP, which seems not to have been an effective assignment.
- **Impacts measured from program start-up.** The Ohio evaluation coincided with program start-up, whereas the California and Florida programs had been in operation for about a year before clients were enrolled in the demonstration.
- **Less aggressive implementation in certain areas.** The Florida program was much more aggressive than Ohio in the use of sanctions, a potentially effective means of increasing participation. In certain California counties, most notably Riverside, intensive job development efforts were undertaken, and may have been partly responsible for the larger impacts in that county.
- **The Ohio sample had a higher proportion of existing ADC cases than in Florida,** where only new and re-opening cases participated in the demonstration.

- **Demonstration entry occurred at a different point in client processing than in California.** In Ohio, clients were randomly assigned at the point they first became subject to JOBS in theory, whereas California only assigned -- and estimated impacts for -- clients who showed after they were requested to appear for a GAIN orientation. Impacts measured for California thus exclude any effects -- or, more likely, non-effects -- for clients who were orientation no-shows.

The strength of the contribution of each of these factors to differences in impacts observed across demonstrations cannot be determined with any precision. It is likely that all played some role.

3.6 DISCUSSION

The overall impacts for Ohio's early JOBS program were quite modest. JOBS consistently generated small positive impacts on employment for early (1989) demonstration entrants, but had little other positive impact. In the case of 1990 entrants, the evaluation found negative impacts for average total earnings during the two observable years of follow-up. There was little evidence of impacts on overall levels of ADC receipt or payments.

Experimental research designs such as the one used in this study can measure the impacts of JOBS quite accurately, but they cannot identify the sources of impacts. We can only speculate about the reasons for our findings, which likely include: modest overall rates of employment and training participation; slow program implementation; difficulties in monitoring clients in JOBS assignments; a programmatic emphasis on activities with long-term payoffs; and a recessionary economy which may have reduced employment prospects through much of the demonstration period. In Chapter 5 we will consider these factors in more depth as possible explanations for the impact findings.

Statistically significant negative impacts for average total earnings appeared to be concentrated in certain subgroups, namely: clients with recent work experience, new ADC clients, and clients from unemployed parent cases. Positive earnings impacts were found for clients with no recent work experience, ongoing ADC clients, and those from single-parent cases, although these effects were not large enough to be statistically significant. Subgroup impacts on ADC payments tended to be small, as in the overall population. Results by family

type were a notable exception: impacts on ADC payments were negative for single-parent families and positive for two-parent families, and in both cases were statistically significant.

Our conclusion -- based on the subgroup findings -- is that the program was most advantageous for those who were not strongly attached to the labor force but who were job ready; for example, those with no recent work experience but with twelve years of school. At the same time, JOBS did little during the demonstration period for those who were already relatively attached to the labor force, or for clients who were not job-ready.

One possible explanation for the subgroup findings is that clients who are prepared to enter jobs but do not have much work experience might need only a small pull to start them on an employment track, a path they would get to on their own only with some difficulty. On the other hand, ADC recipients with relatively strong labor force attachment might be negatively affected by JOBS if program participation weakens their labor force attachment. For example, a person with a history of minimum-wage work might decide to stay on ADC longer than otherwise in order to participate in JOBS and attempt to improve their skills, an effort that may or may not be successful over the long term.

An examination of subgroup differences in employment and training participation also hinted that post-secondary education assignments might partly explain the subgroup patterns. This activity was the one whose utilization was associated most strongly with favorable impacts for subgroups. Just as it is uncertain whether opportunity costs will be rewarded in the long-term, the potential payoffs from an increased emphasis on post-secondary education also are unclear. If clients were required to attend such programs the results might differ from those observed during a period when much post-secondary enrollment was self-initiated. Furthermore, the benefits of the post-secondary strategy necessarily are concentrated among clients who already have a high school diploma.

A final possibility is that clients with greater labor-force attachment tended to receive assignments relatively soon after starting ADC spells, and that their greater opportunity costs (e.g., lower-than-expected earnings) were the result of longer periods in assignments. Our analysis of assignment durations (Fein, 1994) showed that clients who became JOBS-mandatory in 1990 spent 1.7 months longer in assignments than their 1989 counterparts during the first year of follow-up.

CHAPTER FOUR

WORK CHOICE IMPACT FINDINGS

As explained in Chapter 1, each of the major components of the Transitions to Independence Demonstration was fashioned to address the needs of a different segment of the ADC caseload. In the last chapter, we presented impacts for the Fair Work/JOBS program, which required able-bodied clients without young children to participate in employment and training activities. In this chapter, we discuss impacts for a second major demonstration component -- the Work Choice program.

Work Choice was aimed at single-parent (ADC-R) clients whose youngest child was aged one through five. The demonstration operated only in Montgomery County, a large urban area surrounding the city of Dayton. In contrast to Fair Work/JOBS, Work Choice emphasized incentives, supports, and voluntary employment and training participation as routes to economic self-sufficiency. The program reflects a time when many policymakers were hesitant to require mothers with young children to work and receive training outside the home. In view of the risk that some young children might be left in situations where they did not receive adequate care, Work Choice sought to encourage mothers of young children who were most willing and able to find decent child care in making transitions to economic independence.

The incentives provided by Work Choice included extending the duration of Medicaid eligibility for clients exiting ADC for work to 12 months, adding a new entitlement of up to 12 months of transitional child care payments, and offering clients a range of employment and training services. Work Choice's only mandatory element was that clients must participate in employability assessment and counselling.¹

The demonstration represents one of the few tests of extended transition benefits. Unfortunately, the transition benefits component operated within an experimental design only for a short time interval -- January 1989 through March 1990. In April 1990, the State began

¹ Members of the control group remained subject to existing policy, which, prior to the Family Support Act, provided up to only four months of transitional Medicaid and no transitional child care. Control group members also were not assessed or allowed to volunteer for Work Choice employment and training activities, although they could seek such services independently of the program.

providing extended transition benefits to members of the Work Choice control group, in keeping with requirements of the Family Support Act.

On the other hand, control-group members remained ineligible for the mandatory assessment/counselling and voluntary employment and training elements of Work Choice through June 1991. For this reason, and because it likely took some months beyond March 1990 before the control group could learn about transition benefits and respond to any incentive and job retention effects they might have, follow-up is extended through December 1990 in the analyses presented in this report.

This ending point means that, as a group, the maximum possible follow-up for clients who entered the demonstration during the first six months of the full Work Choice program (January through June 1989) was 18 months. The maximum follow-up for those entering during the later months of the demonstration (July 1989 through March 1990) was only nine months. Analyses in this chapter are confined to clients in the second cohort who could be observed for at least twelve months; that is, those entering the demonstration between July and December 1989.²

Restricted follow-up means that the analyses test only hypotheses about short-term program effects. Reflection on hypotheses for major Work Choice components suggests that one should have only modest expectations for the program's impact within this short term.

In the short-run, extended transition benefits might constitute a sufficient incentive to encourage some clients who already were relatively job-ready to seek and find employment. For these, as well as for clients who would have taken jobs even without the additional incentives, transition benefits also might increase job retention and decrease welfare recidivism. Participation in employment and training activities with a short-term employment focus -- such

² As explained in Chapter 2, we analyze impacts separately for these early and later entry cohorts for two reasons. The first is that the former experienced the full Work Choice program on an experimental basis for longer than the latter, given the extension of expanded transition benefits to control group starting as early as April 1990. The second reason for analyzing impacts separately by cohort is that, as in the JOBS demonstration, the two cohorts differed on a number of important characteristics which might affect responsiveness to the treatment. In particular, early Work Choice demonstration entrants were mostly ongoing ADC recipients, whereas later entrants were mostly those starting new spells of receipt.

as job search and employability training -- also could translate into positive short-term impacts on employment and earnings, and negative impacts on welfare use.

Although a number of clients did volunteer for employment-oriented activities, we saw in Chapter 2 that far more opted for intensive activities such as basic and post-secondary education. To the extent that such activities require a longer time to complete, positive impacts on self-sufficiency likely would not be realized within the follow-up period observed in the Work Choice demonstration. In the short-term, participation in educational activities even might lead to negative impacts for some clients who deferred work in order to participate.

Given the possibilities for both positive and negative effects among different client subgroups, it is difficult to make confident hypotheses about the overall direction or magnitude of short-term impacts for Work Choice. The likely influence of the demonstration's mandatory assessment and counselling component is no less ambiguous, since these sessions may have emphasized job search for some clients but intensive education and training for others.

The analysis of Work Choice impacts presented in this chapter employs methods and flows in a manner that closely parallels the analysis of JOBS impacts in Chapter 3. Employment and earnings impacts are presented in Section 4.1, and the corresponding ADC eligibility and payment impacts are given in Section 4.2. In Section 4.3, we present a special analysis of the impacts of Work Choice on transitions from welfare to work. The purpose of this analysis is to examine the extent to which participants took advantage of the offer of extended Medicaid benefits. We present impacts for key client subgroups in Section 4.4 and we conclude in Section 4.5.

4.1 IMPACTS OF WORK CHOICE ON EMPLOYMENT AND EARNINGS

In this section, we present findings on the impacts of Work Choice on clients' employment and earnings. First, we present results for early demonstration entrants -- that is, clients who first became subject to Work Choice between January and June 1989. Then, we examine impacts for later entrants -- those enrolled between July and December 1989.

Results for Early Demonstration Entrants

Exhibit 4.1 presents employment and earnings impacts for the earliest cohort of Work Choice demonstration entrants over the 18-month follow-up period. The format is similar to that used to present impacts for JOBS. The table reveals consistent, modest positive impacts on employment and earnings over the follow-up period.

For the entire 18-month follow-up period, impacts were a positive 3 percentage points for employment and \$177 for average total earnings. Though statistically significant, the impacts are small enough to suggest that Work Choice led to sustained full-time employment for at most a small fraction of clients.

Exhibit 4.2 shows that employment rates and average earnings remained low for both treatment- and control-group members over the follow-up period. By the sixth follow-up quarter, just under four-fifths of both groups were unemployed, and average quarterly earnings hovered near \$300. The chart also highlights the fact that employment and earnings impacts appeared soon after clients entered the demonstration. As noted earlier, services that might lead to rapid emergence of impacts include counselling, job search, and any incentives provided by transition benefits.

Results for Later Demonstration Entrants

Exhibit 4.3 summarizes first-year impacts for clients who entered the Work Choice demonstration from July through December 1989. The exhibit's first two rows give the impacts for employment and average total earnings, respectively.

Results for the later demonstration cohort reveal a statistically insignificant, 3 percentage-point positive impact on employment, and a moderately strong and statistically significant \$192 negative impact on average total earnings.

The negative earnings impact for the later demonstration entry cohort parallels a similar finding for the 1990 JOBS cohort (see Exhibit 3.3). A distinguishing characteristic of both cohorts is that a majority of their clients were just starting ADC spells. In Chapter 3, it was reasoned that other things being equal, such clients might be relatively likely to leave ADC for

Exhibit 4.1

**Impacts of Work Choice on Employment and Total Earnings for
Early (January - June 1989) Demonstration Entrants^a**

Follow-Up Interval	Percent Ever Employed			Average Total Earnings		
	Control Group (1)	Impact ^b (2)	Relative Impact ^c (3)	Control Group (4)	Impact ^b (5)	Relative Impact ^c (6)
Year 1	31.8	3.4**	10.7	\$791	\$109*	13.8
Year 2 (first 6 months)	24.9	3.3**	13.3	516	68	13.2
Entire 18 Months	37.5	3.4**	9.1	1,307	177*	13.5

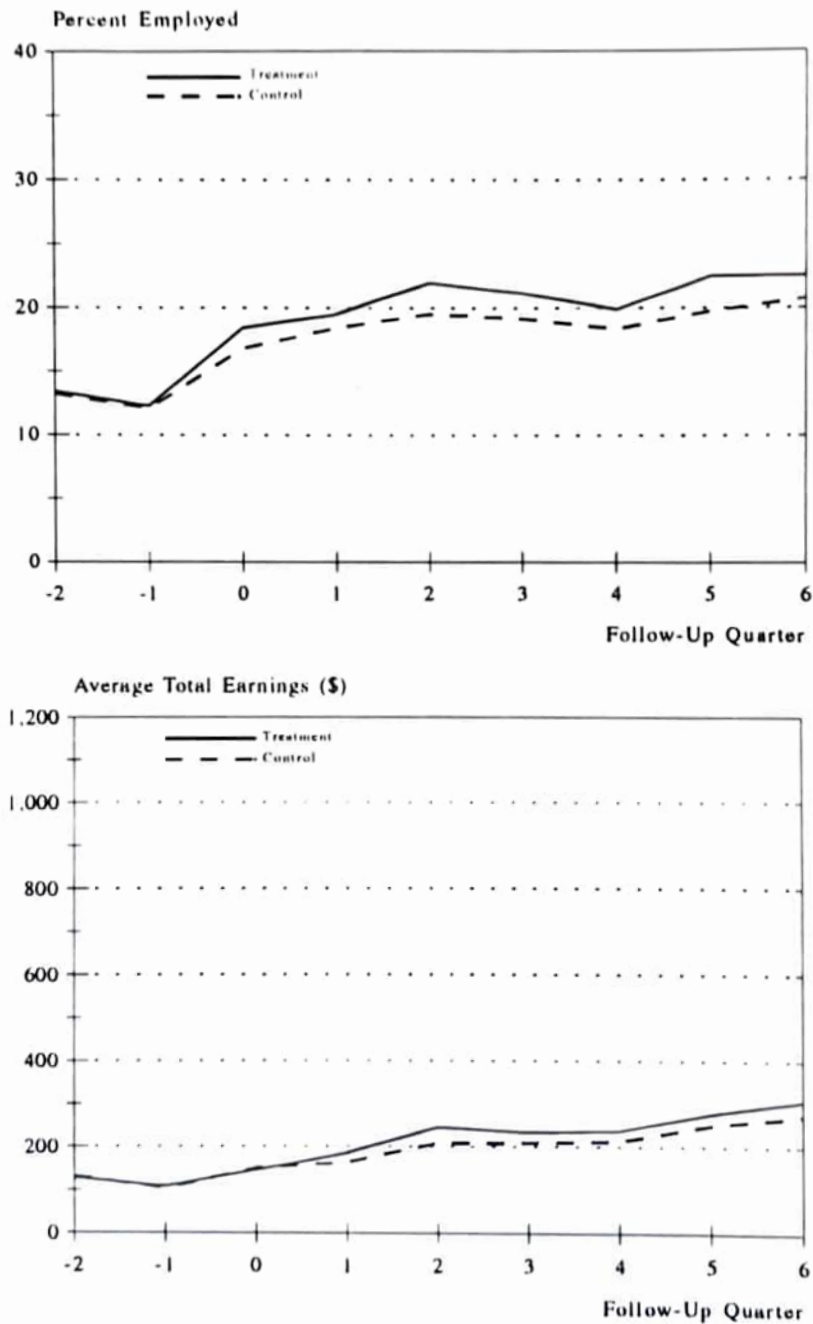
^a Based on a total of 1,619 control and 2,536 treatment group members. Numbers for Year 2 decline somewhat due to outmigration of some sample members from Montgomery County.

^b The impact is the difference between the treatment-group mean (not shown) and the control-group mean (shown in columns 1 and 4).

^c The relative impact is the impact expressed as a percentage of the control-group mean.

*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

Exhibit 4.2
Employment and Average Total Earnings
by Follow-Up Quarter:
Early (January-June 1989) Work Choice Demonstration Entrants



Notes: Quarter 0 is the quarter of demonstration entry.
Quarters -1 and -2 are the two quarters preceding entry.

Exhibit 4.3

**First-Year Impacts of Work Choice for Later (July-December 1989)
Demonstration Entrants^a**

Outcome	Control Group (1)	Impact ^b (2)	Relative Impact ^c (3)
Percent Ever Employed	32.6	3.2	9.8
Average Total Earnings	\$992	-\$192*	-19.4
Percent of Months Received ADC	84.3	-1	-1.2
Average Total ADC Payment	\$3,417	-\$33	-1.0

^a Based on a total of 451 control and 653 treatment group members.

^b The impact is the difference between the treatment-group mean (not shown) and the control-group mean (shown in column 1).

^c The relative impact is the impact expressed as a percentage of the control-group mean.

* Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

work after only a short time on assistance, but that employment and training participation might delay this return.

A further possibility is that the types of assignments received by members of the later cohort were more intensive, and therefore associated with greater opportunity costs, than those received by the early cohort. The analysis of assignment patterns in Exhibit 2.7 supports this hypothesis to some degree, revealing a somewhat higher proportion of education and training activities among assignees in the later than in the earlier cohort.

Although they are not statistically significant, it is nonetheless curious to find a positive employment impact, but negative earnings impact, for the later cohort. We speculate that this finding reflects an impact on the composition of workers. One compositional effect that could explain the result is that Work Choice caused some clients with relatively high earnings potential to defer employment (to participate in program activities), while increasing employment for a somewhat larger group which was able to realize only small earnings gains.

4.2 IMPACTS OF WORK CHOICE ON ADC RECEIPT AND PAYMENTS

In this section, we present estimates of Work Choice's effects on clients' ADC receipt and average ADC payments. Again, we look first at results for the early cohort and then at findings for the later cohort.

Results for Early Demonstration Entrants

The significant positive impacts on employment and earnings for the early enrollees suggest that reductions in ADC dependence may follow. However, as noted in Chapter 3, there is not necessarily a direct relationship here. It takes a relatively large increase in earnings to render clients ineligible for ADC, and the Work Choice population of single mothers with young children seems most likely to work in relatively low-paying, often part-time jobs. Furthermore, small impacts on earnings typically will be accompanied by much smaller, if any, reductions in ADC payment amounts, since a sizable portion could be disregarded as work or child care expense allowances.

Exhibit 4.4 presents impacts for ADC receipt and average payments for the early Work Choice cohort. The estimated impacts on both ADC receipt and payments are small and statistically insignificant. The quarter-by-quarter estimates shown in Exhibit 4.5 confirm the absence of impacts. There is no perceptible difference between treatment- and control group statistics in either panel. Clients in both groups exhibit a gradual reduction in ADC use, consistent with normal welfare dynamics. By the end of the sixth quarter of follow-up, nearly 25 percent of clients were no longer on assistance.³

Results for Later Demonstration Entrants

The bottom two rows of Exhibit 4.3 display the corresponding ADC impacts for the later Work Choice entry cohort. Like the results for early enrollees, the impacts are small and statistically insignificant.

4.3 IMPACTS ON TRANSITIONS FROM ADC TO WORK FOR THE EARLY COHORT

Although Work Choice produced no net impacts on welfare dependency, it still might have had more subtle impacts on welfare dynamics. For example, the program might have caused counterbalancing increases in clients' movements off and back onto welfare. The net impacts do not close the door on the possibility that Work Choice encouraged some clients to leave ADC faster than they would have otherwise, and/or changed the reasons why clients left assistance.

The circumstances surrounding ADC exits are of special interest in the Work Choice demonstration, given the offer of extended transition benefits. On a purely descriptive level, we would like to know, compared with the control group, how many members of the treatment group took advantage of extended benefits; and have some basis for relating these impacts to those for all ADC exits and all exits to employment.

³ The 75 percent of enrollees still on ADC at the end of quarter six includes those who left ADC at some point but later returned to assistance.

Exhibit 4.4

**Impacts of Work Choice on ADC Receipt and Total ADC Payments
for Early (January - June 1989) Demonstration Entrants^a**

Outcome and Follow-Up Interval	Percent of Months Received ADC			Average Total ADC Payments		
	Control Group (1)	Impact ^b (2)	Relative Impact ^c (3)	Control Group (4)	Impact ^b (5)	Relative Impact ^c (6)
Year 1	86.5	.3	.3	\$3,390	-\$1	-.03
Year 2 (first 6 months)	75.9	.6	.8	1,553	11	.7
Entire 18 Months	83.2	.4	.5	4,961	14	.3

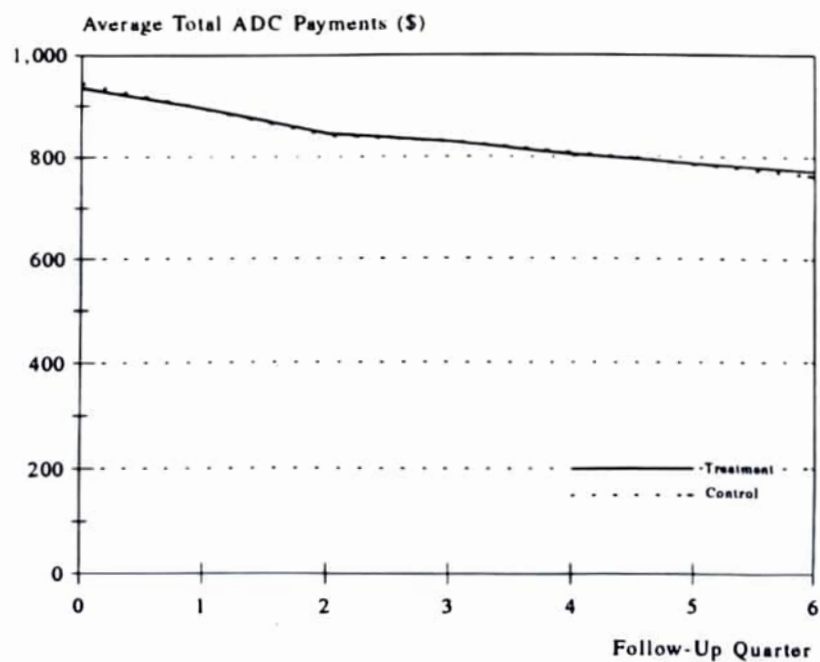
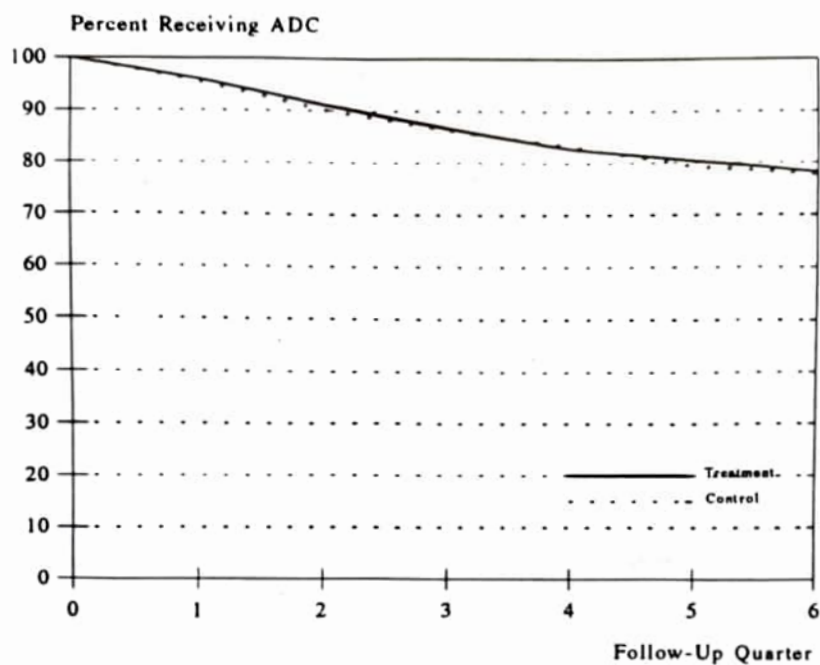
^a Based on a total of 1,619 control and 2,536 treatment group members. Numbers for Year 2 decline somewhat due to outmigration of some sample members from Montgomery County.

^b The impact is the difference between the treatment group mean (not shown) and the control-group mean (shown in columns 1 and 4).

^c The relative impact is the impact expressed as a percentage of the control-group mean.

*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

Exhibit 4.5
ADC Receipt and Average Total Payments
by Follow-Up Quarter:
Early (January-June 1989) Work Choice Demonstration Entrants



Note: Quarter 0 is the quarter of demonstration entry

Such an analysis remains descriptive and not explanatory, since a finding that Work Choice increased ADC exits to employment with extended transition benefits does not necessarily imply that the extended benefits incentive was the operative mechanism. As noted previously, other Work Choice components which might produce short-term employment impacts include mandatory counselling and voluntary participation in employment-directed activities.

The following three sections present results from an analysis of treatment- and control-group members' cumulative rates of exiting ADC. The first section shows cumulative rates of exit for all reasons. The second section shows rates of exit to employment. Finally, the third section shows rates of exit to employment with extended Medicaid.

ADC Exits Overall

Exhibit 4.6 plots the cumulative percentage of all Work Choice treatment- and control-group members ever leaving ADC over time. By the sixth quarter of follow-up, more than two-fifths of all sample members had experienced at least one month in which they did not receive ADC payments.⁴ Thus, over half of the enrollees (about 55 percent) received monthly ADC payments continuously over the year and a half of follow-up. There are virtually no differences in ADC exit rates between treatment- and control-group members. This finding suggests that the insignificant net impacts on ADC receipt seen in Exhibits 4.4 and 4.5 were not masking positive impacts on rates of ADC exit and return.

ADC Exits to Employment

The top panel of Exhibit 4.7 shows the cumulative percentage of early entrants ever leaving ADC for employment. We define an exit to employment as a quarter in which a case

⁴ The top panel of Exhibit 4.5 indicated that in the last quarter of follow-up, about 25 percent of enrollees were off ADC. Exhibit 4.6 shows that 50 percent of clients ever got off ADC during the 18 months of follow-up. Together, the exhibits suggest a rate of return back to ADC, or recidivism rate, of 50 percent. That is, half of those who exited ADC at some point during follow-up were receiving ADC in the last quarter.

Exhibit 4.6
Cumulative Percent of Early (January-June 1989) Work Choice
Demonstration Entrants Ever Exiting ADC

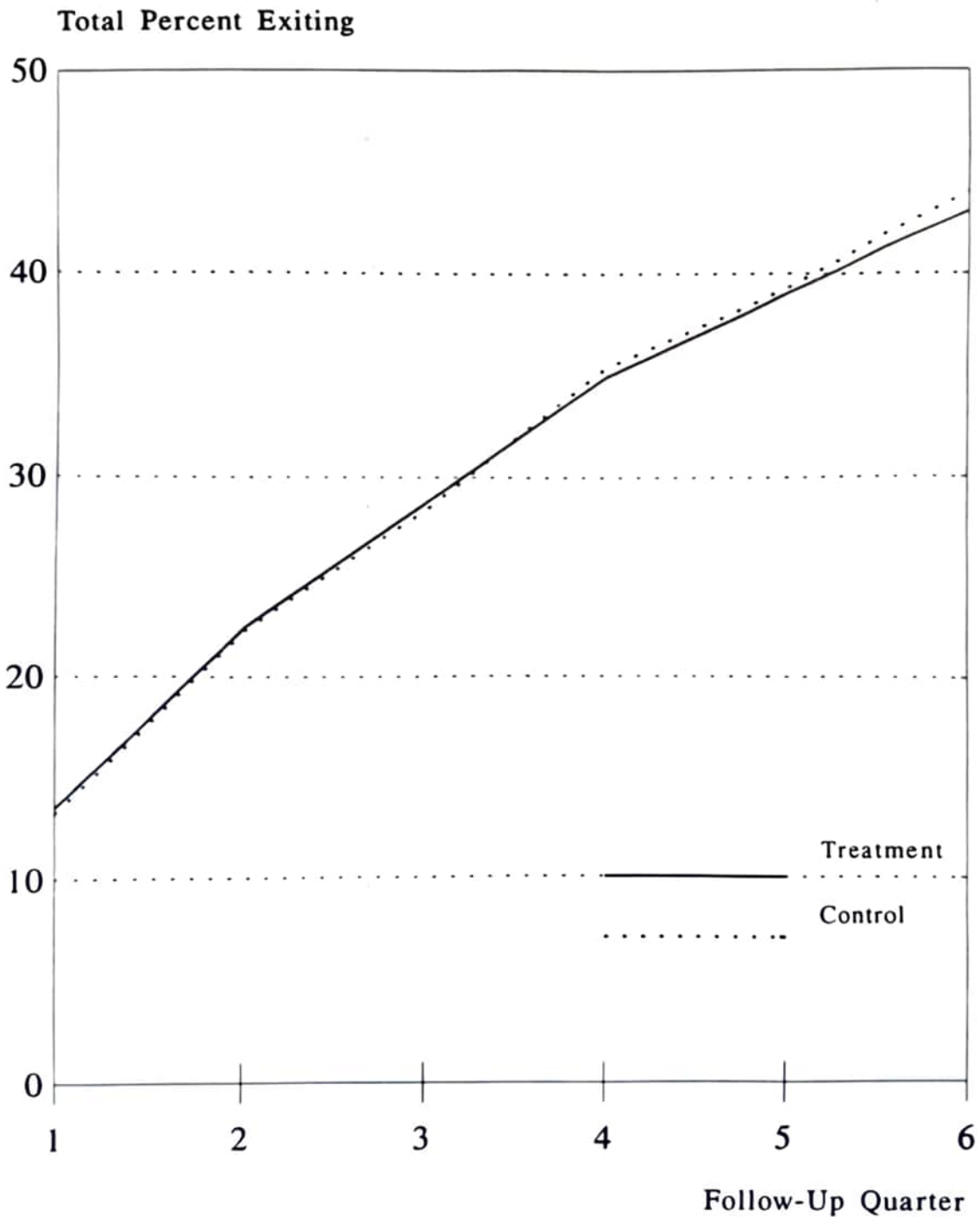
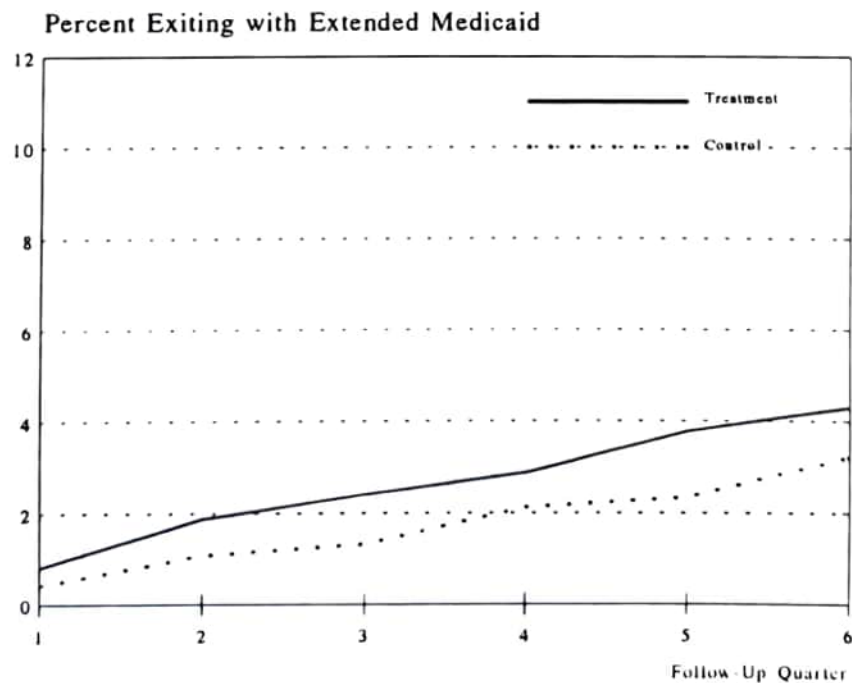
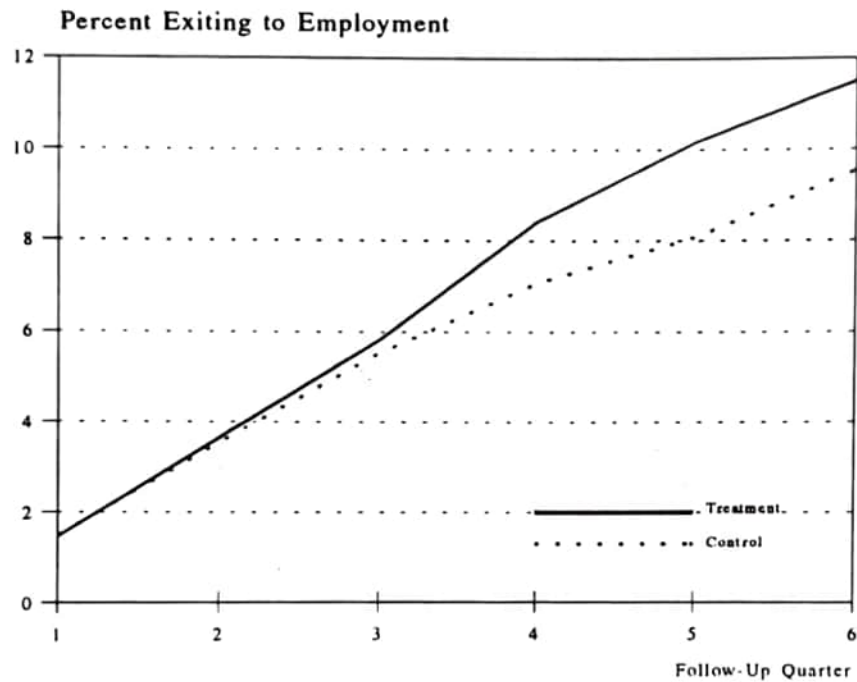


Exhibit 4.7
Cumulative Percent of Early (January - June 1989) Work Choice
Demonstration Entrants Ever Exiting ADC
to Employment and Extended Medicaid



is not receiving ADC payments and either has positive earnings in that quarter or positive earnings in the quarter immediately after.

The exhibit indicates that, so measured, exits to employment account for 27 and 22 percent of all ADC exits in the treatment and control groups, respectively. The true fraction of employment-related exits could be either higher or lower than this, as our measure only approximately identifies work-related exits. Some exits will be mis-attributed to employment where work was not the main reason, and some exits will be missed if employment was not covered by the Unemployment Insurance system.⁵

We find that, even without affecting overall ADC exits, Work Choice did have a positive, statistically significant impact on work-related exits. The impact emerged in the third follow-up quarter and had reached two percentage points by the end of the sixth follow-up quarter.

How could Work Choice have had a positive impact on work-related exits from ADC, without affecting the total percentage of enrollees exiting ADC? One possibility is that it changed the circumstances surrounding leaving ADC for clients who would have left ADC anyway. Counselling, job services, and/or transition benefit incentives may have motivated some clients to try working who would otherwise have relied only on partners or family members for financial support. A second possibility is that Work Choice changed the *types* of people who left ADC in a way that did not affect overall rates of exit. For example, the program may have convinced some people who would have left because of marriage to stay on ADC (perhaps to pursue education and training), while inducing others who otherwise would have stayed on ADC to work.

ADC Exits with Transitional Medicaid Benefits

Both treatment- and control-group members were eligible for transitional Medicaid: the only difference was in the length of time the two groups could receive these benefits (twelve and

⁵ Such problems do not necessarily affect the accuracy of estimated impacts, since the biases could be equivalent (and hence cancelling) for cases in the treatment and control groups. (See Bloom et al. 1993, Appendix E, for evidence that biases do cancel.)

four months, respectively). Thus, it is of interest to analyze the effects of the total Work Choice package (including any greater incentives from extended Medicaid) on exits with Medicaid. Such exits were measured using data on Medicaid eligibility from the CRIS and CRIS-E systems.

The bottom panel of Exhibit 4.7 shows the cumulative percentages of treatment- and control-group members leaving ADC with transitional Medicaid benefits. The transitional Medicaid exit rates are quite low: only four percent of clients in the treatment group and three percent of those in the control group ever claimed transition benefits over the first 18 months of follow-up. Although small, the treatment-control difference is statistically significant.

Comparing the graphs in the top and bottom panels of the exhibit, one sees that a majority of clients leaving welfare for work did not receive transitional Medicaid. The fraction may be even greater than is apparent here, given that our measure of all ADC exits to employment missed exits to employment not reported to the U.I. system. The data do not allow us to measure the extent to which those who did not get extended Medicaid received health insurance from their employers.⁶

Summary

Although there were no statistically significant impacts on net ADC receipt or payments, Work Choice did slightly increase the rate at which clients left ADC for work. The demonstration also generated a small positive impact on the percentage of clients who ever claimed extended Medicaid benefits. The analysis does not reveal whether the offer of extended transitional Medicaid was responsible for the observed employment impacts, since the impact on Medicaid exits could have a variety of causes. Overall, the percentage of Work Choice-eligible clients in the treatment group who ever received extended Medicaid was, at four percent, quite small.

⁶ Another difference is that impacts on exits with extended Medicaid occur almost immediately, whereas impacts on all employment-related exits take several quarters to appear. Disparities in data quality between the U.I. system and CRIS/CRIS-E likely explain these differences.

It is interesting that Work Choice increased the number of clients leaving ADC for work, but produced no impact on overall exits from ADC generally. The Work Choice program affected the reasons for leaving ADC, and possibly the composition of exiters, but did not change the overall rate of departure among all clients.

4.4 IMPACTS ON KEY CLIENT SUBGROUPS IN THE EARLY DEMONSTRATION ENTRY COHORT

In this section, we examine impacts for subgroups within the Work Choice population. F-tests revealed that none of the impacts differed significantly among the subgroups tested at the 10-percent confidence level. However, magnitudes and significance tests for point estimates often did differ in interesting ways. A number of the differences bear a close similarity to subgroup findings in the JOBS analysis: this similarity strengthens our belief that the results are worth reporting.

Subgroups examined in the most detail are defined with reference to their recent work experience, time in their current welfare spell, and level of education at baseline. In addition, we briefly discuss results for subgroups defined by client's age and number of children at demonstration entry. Chapter 3 provides more detail on the rationale and methodology behind the subgroup analyses.

Recent Work Experience

Impacts varied according to whether clients had recent work experience, as defined by the presence of any earnings in the two quarters before demonstration entry. The top panel of Exhibit 4.8 reveals strong similarities to the subgroup results seen for JOBS. Work Choice impacts on total earnings were positive for clients who did not have recent work experience and negative for those who did have recent work experience. For the last six months of follow-up, impacts on average total earnings were \$108 for those with, and -\$115 for those without, recent employment. Also mirroring the JOBS results, Exhibit 4.8 shows little evidence of subgroup differences in impacts on ADC payments by work experience.

Exhibit 4.8

Impacts of Work Choice on Total Earnings and Total ADC Payments for Key Subgroups:
Early (January - June 1989) Demonstration Entrants

Subgroup and Follow-up Interval	Average Total Earnings			Average Total ADC Payments			Sample Size	
	Control Group (1)	Impact* (2)	Relative Impact ^b (3)	Control Group (4)	Impact* (5)	Relative Impact ^b (6)	Control (7)	Treatment (8)
<u>Ever Employed in Six Months Before Demonstration Enrollment?</u>								
No								
Year 1	\$398	\$156**	39.2	\$3,426	\$3	.1	1,289	2,003
Year 2 (First 6 months)	311	108**	34.7	1,539	12	.8		
Yes								
Year 1	2,374	-131	-5.5	3,184	42	1.3	330	533
Year 2 (First 6 months)	1,353	-115	-8.5	1,554	47	3.0		
<u>Duration of ADC Receipt in Current Spell</u>								
New/Re-Opened Case								
Year 1	\$934	\$30	3.2	\$2,646	\$64	2.4	170	295
Year 2 (First 6 months)	626	-119	-19.0	1,170	-44	-3.8		
Less Than Two Years								
Year 1	766	120	15.7	3,116	19	.6	528	762
Year 2 (First 6 months)	501	93	18.6	1,407	65	4.6		
Two Years or More								
Year 1	797	96	12.0	3,661	-4	-.1	921	1,479
Year 2 (First 6 months)	514	79	15.4	1,685	6	.4		
<u>Years of School Completed at Demonstration Enrollment</u>								
Less than 12								
Year 1	\$556	\$29	5.2	\$3,603	-\$32	-.9	686	1,114
Year 2 (First 6 months)	404	-26	-6.4	1,690	-9	-.5		
12 or more								
Year 1	982	148*	15.1	3,210	43	1.3	933	1,422
Year 2 (First 6 months)	606	128**	21.1	2,434	40	2.8		

Note: F-tests were performed to detect differences in impacts among categories within each subgroup for each year of follow-up. No significant differences were found.

* The impact is the difference between the treatment-group mean (not shown) and the control group mean (shown in columns 1 and 4).

^b The relative impact is the impact expressed as a percentage of the control-group mean.

* Impacts statistically different from zero at the 10% level; ** at the 5% level; *** at the 1% level.

In Chapter 3, we speculated that differences in impacts for clients with less and more work experience might reflect differences in alternatives to program participation available to these two groups. Whereas clients with less experience would be relatively unlikely to work absent the program (and thus have little earnings to lose), those with greater labor force attachment could be expected to be more likely to defer work in favor of education and training participation. This explanation also seems reasonable for Work Choice, especially given that intensive activities were even more prominent among employment and training assignments than in JOBS.

Duration of Current ADC Spell

The middle rows of Exhibit 4.8 display impact estimates for subgroups distinguished by the length of clients' ADC spells at the point they entered the Work Choice demonstration. None of the subgroups reveal statistically significant impacts for either earnings or ADC payments.

On a purely descriptive level, however, the pattern here also resembles the corresponding JOBS subgroup findings, in that earnings impacts are smallest (indeed, negative in Year 2) for clients just starting ADC spells (in new or re-opening cases). This finding, too, may reflect somewhat greater opportunity costs for new recipients, who, on average, can be expected to have greater work experience.

Education

In Chapter 3, we reported positive impacts on average total earnings among clients with 12 or more years of education, and small and statistically insignificant impacts for those with less than 12 years of education. Exhibit 4.8 shows similar result for the Work Choice sample. Earnings impacts for Year 1 and the first six months of Year 2 were a positive \$148 and \$128, respectively, among clients with 12 or more years of education; and statistically insignificant for those with less education. As in JOBS, no statistically significant ADC payment impacts are evident.

The lack of positive impacts for clients with less than 12 years of education suggests that raising the earnings of poorly-educated clients is a difficult task. Even when such clients are highly motivated, their starting disadvantages are such that considerable time may be needed to achieve self-sufficiency. In contrast, clients who already have a high school diploma are in a better position to respond to short-term activities such as job search and job readiness training. Better-educated clients also are hypothesized to be in a better position to respond quickly to any incentives contained in extended transition benefits.

Other Subgroups

Young welfare recipients are frequently faced with a combination of obstacles that make self-sufficiency especially elusive. They tend to have low levels of education and work experience, greater child care and health care needs given the young ages of their children, and poor life planning skills. Several major welfare reform plans currently under discussion would concentrate resources on younger recipients in the hope that the intergenerational cycle of welfare dependency could be ended.

To see whether Work Choice had different effects for clients who were under age 25 and those over 25, we estimated impacts separately for these groups. Differences in impacts were exceedingly small and statistically insignificant. This finding provides little basis for concluding that efforts focussed on youth will be any more or less successful than those aimed towards adults.

We also examined impacts for subgroups differentiated according to whether the youngest child was under age three or older. We thought that those with younger children might be harder to serve because they faced greater child care constraints and were less inclined to participate. However, again, we found no differences in impacts between these groups that were either statistically or substantively significant.

Summary

The impacts for the subgroups we have examined here are broadly consistent with the findings for the JOBS program, as well as with the results of several previous evaluations of state welfare demonstrations (see Friedlander 1993). Our findings suggest that the most favorable results for clients occur at moderate levels of disadvantage. Specifically, we find that earnings gains were concentrated among participants with higher levels of educational attainment but lower levels of work experience.

4.5 DISCUSSION

The analyses presented in this chapter have shown that Work Choice had small positive effects on employment and earnings for the earliest demonstration entry cohort. These impacts were not large enough to translate into reductions in ADC receipt or payments. Results for later demonstration entrants were less favorable: we found a modest negative impact on average total earnings.

The nature of the experiment does not allow us to isolate the extent to which each of the main components of Work Choice contributed to the short-term impacts that were observed. For example, the positive employment and earnings impacts which were detected might be the result of mandatory counselling, the incentive that transition benefits offer to enter and keep jobs, participation in activities with an immediate focus on employment (such as job search and job readiness), or a combination of these factors.

In interpreting the Work Choice impacts, it is important to remember that only limited follow-up was possible. Due to the premature ending of the demonstration's transition benefits component, it was not possible to observe study subjects for more than 18 months. It is quite possible that impacts would take longer than this to emerge, given that recipients tended to volunteer for intensive educational assignments and that these assignments often were not made until some time into the follow-up period.

CHAPTER FIVE

CONCLUSIONS

Six years after launching the Transitions to Independence Demonstration, Ohio now stands at the brink of a new round of welfare reform. Its three new welfare reform initiatives -- Communities of Opportunity, Families of Opportunity, and Children of Opportunity -- go substantially beyond the employment and training strategy. The new policies test new incentives to work; expansions in supportive services; incentives to family stability; broadening of the ages of children subject to learnfare; employment subsidies; and economic development.

The latest Ohio reforms embody a growing national sense that services created by the Family Support Act are not enough to substantially end the need for welfare. This sense appears to be grounded both in the direct observation that welfare caseloads do not appear to have been much affected by JOBS and related services during the program's early years, and by early evaluations finding that impacts were little better than the modest results of predecessor employment and training initiatives.

Many observers argue that the program's early years must not be judged too harshly. During these years, state and federal funding for the Family Support Act never approached the levels required for full implementation. Clients emerging from JOBS employment and training activities in the midst of a deep recession also faced an unusually difficult job market during this era.

Still, the findings presented in this report tend to reinforce the general consensus that, by itself, the employment and training strategy -- whether mandatory or voluntary -- so far has produced quite modest results. Neither JOBS nor Work Choice produced statistically significant overall impacts on ADC participation or payments. Although the two programs did have impacts on employment and earnings, these effects were quite modest and not always positive in direction.

Consideration of the Ohio evidence does not necessarily lead to the conclusion that JOBS and Work Choice were based on faulty premises about what welfare recipients need to become self-sufficient. One alternative hypothesis is that difficulties with program implementation,

rather than deficient program design, limited the magnitude of the impacts. Another possibility is that employment and training and supportive services are *necessary* components of an effective approach but not *sufficient* in themselves to help a substantial portion of recipients leave welfare. Even under the best economic conditions, it is hard for welfare recipients to find jobs whose compensation compares favorably with welfare benefits (including Medicaid and food stamps). The recession which prevailed through most of the demonstration years (1989-92) both limited job availability and diluted JOBS resources, which had to be spread across an expanded caseload.

Both interpretations offer reasons to be hopeful that JOBS and related services could lead to stronger positive impacts in Ohio in the future. Indeed, to the extent that the State has moved past early implementation problems and out of the recession that characterized the years covered by this study, its JOBS program already may be generating more favorable outcomes.

Even during the difficult years in which Ohio's JOBS and Work Choice programs were implemented, the programs were able to help many clients improve their skills and earnings. In the next two sections of this chapter (5.1 and 5.2), we observe that the impacts of JOBS and Work Choice were more favorable for some subgroups and E&T assignments than others.

In Section 5.3, we identify some of the likely reasons why program impacts were not greater, distinguishing aspects of program design and implementation which seem to have been problematic. In a brief closing section, we note that, given the slow implementation of JOBS over the study period and restricted follow-up opportunity for Work Choice, the jury may still be out as to the long-term promise of these two approaches.

5.1 VARIABILITY IN IMPACTS ACROSS SUBGROUPS

Belying the small overall impacts, stronger impacts on total earnings -- both positive and negative -- were found for a number of subgroups in both the JOBS and Work Choice demonstrations. Such findings are important because they reinforce the message that modest overall impacts may hide important impacts for some subgroups and services. The general tenor -- if not the exact magnitudes -- of subgroup impacts were surprisingly consistent for JOBS and Work Choice.

The subgroup analysis showed that positive impacts on earnings tended to be concentrated among relatively disadvantaged clients, including clients who had not worked in the six months preceding demonstration enrollment, ongoing (as opposed to new) recipients, and (for JOBS) single parents. An exception to this finding was that impacts also were positive for clients with 12 or more years of completed education.

It must be emphasized that the magnitudes of these positive impacts were modest: in no case did they exceed \$500 over an entire year, nor -- save for heads of single-parent cases in JOBS -- were they accompanied by statistically significant reductions in ADC payments. Nonetheless, the subgroup analyses do suggest that JOBS and Work Choice increased earnings within some important client categories.

The least encouraging results appeared for clients with greater labor force attachment (including those with recent work experience, first-time applicants and re-enrollees, and clients from unemployed parent cases) and clients with less than 12 years of completed education. Impacts for these subgroups generally were insignificant or negative, up to a third-year earnings impact of -\$776 for new JOBS cases.

The finding that impacts were greatest for clients with less immediate labor force experience but higher levels of education reinforces previous research showing that employment and training programs tend to help moderately disadvantaged clients more than the most or least disadvantaged ones (Friedlander 1993). The most disadvantaged clients present greater challenges because they may have very little motivation and/or because their job skills are poor. On the other hand, relatively advantaged clients may have less need for employment and training services.

Indeed, to the extent that clients with greater labor force attachment are encouraged to participate in activities that are not directly focussed on job search and job placement, programs like JOBS and Work Choice actually may divert them from work. The negative employment and earnings impacts seen for some subgroups with greater labor force attachment does suggest there were such opportunity costs. The subgroup findings do not provide consistent evidence on whether these opportunity costs might be a temporary sacrifice en route to improved long-term employability or a persisting consequence of weakened labor force attachment.

In addition to varying opportunity costs, informal analyses suggest that the most favorable results also appeared for subgroups with the highest rates of assignment to post-secondary activities. The findings characterize an environment where post-secondary assignments were largely client-initiated -- rather than mandatory. Nonetheless, there may be room for strengthening overall impacts by expanding use of this assignment.

5.2 EFFECTIVENESS OF DIFFERENT E&T ASSIGNMENTS

The subtlety of program impacts in Ohio's JOBS and Work Choice demonstrations also is suggested by findings from non-experimental analyses which show that the effectiveness of different E&T assignments was not uniform.¹ These analyses reveal that assignments to job club, post-secondary education, and job readiness activities produced statistically significant positive third year impacts on employment and earnings, whereas assignments to basic education and CWEP led to negative and insignificant impacts, respectively. Numerically, basic education and CWEP assignments represented a majority of JOBS assignments during the study period, suggesting that the program's overall effects may have been constrained by the prevalence of these two assignments.

The poor showing for basic education in the non-experimental analyses is consistent with the finding from the experimental analysis showing negative impacts for clients with less than 12 years of education. Basic education is the main distinguishing assignment for this group, compared with clients with 12 years of education or more. In the next sections, we discuss the extent to which the poor basic education results reflect attributes of clients or the ways in which services were provided.

Examination of the pace of first assignments in Chapter 2 suggests that slow program implementation also may have contributed to the modest results. A relatively large fraction of first JOBS assignments did not occur until more than a year after demonstration enrollment,

¹ The methodology in these non-experimental analyses involved estimating regression equations for employment and earnings for the control group and then using these to predict, for each treatment-group member, what she would have earned absent JOBS. Average predicted and actual earnings were then estimated for clients receiving different JOBS employment and training services. Details will be found in Fein (forthcoming).

implying that many clients were not long in assignments at the point that impacts were observed. In the non-experimental analyses, we estimated impacts separately for clients who received their first assignments during their first year of demonstration enrollment and found that positive impacts emerged much more quickly for these than for all clients.² Such findings offer some hope that the effects of JOBS might intensify over a period longer than that observed in this evaluation.

5.3 PROGRAM DESIGN AND IMPLEMENTATION ISSUES

It is a long step from reviewing impacts for different subgroups and E&T assignments to understanding why impacts were not larger. The research design does not allow us to measure the relative importance of different factors: the best we can do is identify the possibilities and assemble impressionistic evidence on their likely roles from our process study and the wider evaluation literature.

The following sections identify a number of aspects of program design and implementation which may provide opportunities for strengthening impacts. In general, questions about design go to the essential philosophy and approach of the intervention, whereas problems with implementation are concerned with how this vision was translated into an operational program. It is important to distinguish these fundamental causal possibilities, as they can imply quite different remedial actions.

Program Design

In this section, we note four basic aspects of the design of Ohio's JOBS and Work Choice programs about which questions may be raised. One is the use of a flexible choice model for determining employment and training assignments. Another is the mix of assignments emphasized. A third issue is whether transition benefits were ample enough to provide the

² Although employment and earnings impacts for clients ever assigned to basic education during the first year remained negative, their magnitudes declined substantially over the three years observed.

intended incentives and support. A fourth issue is the most basic: whether the essentially "supply side" approach represented by the employment and training/supportive services strategy -- which concentrates on increasing the employability of a supply of potential workers -- is sufficient in light of constraints in the labor market for low skill workers.

Choice Model

A distinguishing aspect of Ohio's JOBS and Work Choice programs was that they gave caseworkers considerable discretion in determining employment and training assignments. The process study found that the design also assigned heavy weight to clients' assignment preferences.

The choice model puts heavy responsibility on caseworkers' shoulders. It likely works best in a well-developed case management environment, where caseworkers have appropriate training, reasonable caseloads, and sufficient placement and support resources. For most of the period covered by this study, these conditions were not present.

In some cases, clients' employment and training assignment preferences may lie along the most effective route to long-term self-sufficiency. However, the process study suggests that some clients in mandatory programs like JOBS will opt for assignments that require the least effort to fulfill, rather than those that would be most beneficial.

Mix of E&T Assignments

As summarized earlier, non-experimental analyses showed that two prominent employment and training activities -- basic education and CWEP -- were associated with the most modest results. In itself, these findings do not immediately lead to a course of remedial action, as they could point to problems as different as: 1) faulty assumptions about clients' needs, 2) the fact that it requires much time for the most disadvantaged clients to become employable, or 3) the nature of the basic education classes and CWEP positions offered.

The evaluation produced some evidence that factors 2 and 3 contributed to the lackluster effects of basic education. Consistent with the distance-to-go hypothesis(2), the non-experimental results show signs of slow positive movement in impacts over time for basic education assignees, especially those assigned during the first 12 months after demonstration

enrollment. With regard to how basic education classes are structured, information from the process study indicates that the State's heavy reliance on existing resources -- as opposed to developing programs tailored to the needs of welfare clients -- may not have produced optimal educational environments.

More basic questions have been raised about whether basic education may be, in the words of Herr and Halpern, "a problematic first step" for welfare clients with a history of negative experiences in traditional school environments (Herr and Halpern 1991). An early emphasis on vocational training and/or work experience could lead to a better appreciation of the need for education, as well as directly enhance employability (Burghardt et al. 1992). On the other hand, until the educational system offers a credible training/apprenticeship route as an alternative to traditional academic credentialing, it is likely that persons without at least a high school degree will continue to face extremely poor employment prospects. Thus, although the design of basic education classes probably could be improved greatly, there is little evidence that society can afford to neglect clients' basic educational needs.

In contrast, it seems less clear that work experience was always appropriate for the kinds of clients receiving CWEP assignments in Ohio. In general, clients in the best position to benefit from CWEP are those who possess basic educational skills but lack work experience. The potential gains for such clients will be greatest when they are placed in carefully-structured and supervised settings and given opportunities to acquire marketable skills. Information from the process study suggests that these conditions rarely prevailed in Ohio. CWEP often was used as an assignment of "last resort" for clients who had refused basic education assignments (and presumably had poor basic skills) or had high school diplomas but were unable to find jobs after participating in job club and other activities. Both caseworkers and clients generally viewed these assignments more as fulfilling an obligation to "work off their grants" than as opportunities to develop new skills. As noted in the discussion of implementation issues, the process study also found that CWEP assignments tended to be poorly monitored and were plagued by poor attendance.

Given these findings, it is perhaps not surprising that the non-experimental analysis revealed no tendency for CWEP impacts to grow more positive over time. In view of these results, and consistently poor results for CWEP in other states (Brock *et al.* 1993), the basic

strategy seems open to question. The use of CWEP as a means to meeting the stricter "work activity" participation standards for unemployed parents seems especially questionable, given that unemployed parent eligibility rules already guarantee that such clients have recent work experience.

Transition Benefits

Work Choice results indicate that providing up to 12 months of transitional child care and Medicaid had at most only a small positive impact on transitions from welfare to work. We hasten to note that the experiment afforded at best a limited test: transition benefits might have led to larger net employment and earnings impacts if participants could have been observed longer than was possible in Work Choice. In particular, longer follow-up would have provided a better test of the combined effects of transition benefits and voluntary employment and training participation. The 14 percent of Work Choice eligibles that volunteered for employment and training tended to opt for the most intensive activities -- basic and post-secondary education -- and may still have been in these activities through much of the follow-up period.

Of course, the small impacts also could imply that transition benefits provided a weak incentive: clients may have perceived them to offer only a slight additional cushion compared with existing benefits. Benefits may have to be extended for a longer period before clients perceive them to offer a solid foundation for low-wage employment. A number of states -- including Ohio -- currently are implementing or planning demonstrations which test longer extensions of transition benefits.

It is possible that transition benefits would have stronger impacts on job retention than on job taking. Unfortunately, the limited follow-up period and the small number of clients who took advantage of these benefits precluded estimating job retention effects.

Basic Limits to the Employment and Training Strategy

One final "design" issue is the possibility that the poor quality of jobs available to welfare recipients in the low-skill end of the labor market acts as a strong constraint to the effectiveness of the employment and training strategy. Such a conclusion is reinforced by the finding that no

employment and training program thus far has produced a large reduction in welfare dependency.

These "demand side" difficulties were especially acute during the period covered by the Transitions to Independence Demonstration, when Ohio and the rest of the nation was undergoing one of the most serious recessions of recent times. By most accounts, the inherent difficulties of implementing JOBS were exacerbated greatly by caseload increases which brought a surge in the number of mandatory clients and an economy which offered fewer jobs. As Bane and Ellwood note:

In slack economies with high general unemployment rates, job placements are much more difficult. In such situations, participation requirements may actually lengthen stays on welfare, if recipients enroll in successive education or training programs. The early 1990s were years of recession and stagnant recovery, as well as fiscal crisis, in most states...[E]xperience with the JOBS program thus far, as with earlier employment and training efforts, underscores the inevitable limitations of such an approach taken by itself (1994, pp. 26-7).

The "supply side" of the equation also may present serious constraints, in that there likely is a segment of the caseload who will prove extremely difficult to employ. This segment includes clients with deep-set attitudinal problems or mental or physical health problems who resist the best efforts of caseworkers and service providers to motivate and teach. There likely are numerous clients who do not qualify for official JOBS exemptions but strongly resist program services.

Implementation

JOBS and Work Choice were complex new programs, which required ODHS and counties to develop a broad range of services, and new procedures and systems for keeping track of participants. Ohio's approach to implementation reflects the substantial autonomy of county governments in that state, as well as the State's strong legacy of experience with CWEP. As in many other states, implementing JOBS was an incremental process. In this section, we comment on four aspects of implementation which may have constrained impacts.

Processing the Mandatory Caseload

The slow pace at which clients were assessed and assigned to employment and training activities throughout much of JOBS's first four years likely generated one of the most basic constraints to program impacts. Chapter 2 showed that a high proportion of assignments occurred more than a year after clients first became JOBS-mandatory.

The slow pace with which clients were processed into Ohio's JOBS program was the consequence of gradual program implementation in the face of a high volume of existing and new mandatory clients requiring services. As in other states, the struggle to staff CDHS JOBS units and provide necessary program services adequately continued through most of the early years of JOBS.

Large backlogs of mandatory clients requiring services were chronic through much of the demonstration period in Ohio. Decisions on how to prioritize clients in the backlog were left largely to the CDHSs, which adopted a variety of ad hoc solutions to the problem. Rather than selecting on client needs or characteristics, CDHSs generally attempted to call in and assess as many recently referred clients as staff could handle. This approach generated high caseworker burden and left little time for in-depth case management services. Although ODHS did attempt to explain the national JOBS targeting requirements to counties, it did little to see that targeting actually was carried out.

Providing Employment and Training Services

By one measure -- the share of Ohio's federal JOBS allocation that was claimed -- the State seems to have been relatively successful at obtaining services for JOBS participants. Between 1989 through 1992, Ohio claimed a substantially higher share of its federal JOBS allocation than did the nation as a whole.³ This may be one reason why JOBS participation rates were somewhat higher in Ohio than for the nation as a whole. Still, the absolute magnitudes of participation rates remained quite low in Ohio, as in other states, throughout the early years of JOBS.

³ For example, in 1992, when most states had implemented JOBS statewide, Ohio claimed 85 percent of its federal allocation, compared with 68 percent nationwide.

The process study raised a number of questions about the quality as well as quantity of JOBS services. Basic education and CWEP placements appeared to be consistently the most problematic JOBS components. Ohio generally followed the strategy of mainstreaming welfare clients into regular adult basic education classes. While sensible from the standpoint of maximizing use of existing resources, the process study found that the environments in these classes often were not appropriate to the special needs and circumstances of welfare recipients. Rather than being carefully developed to provide opportunities to acquire useful skills, CWEP positions generally were viewed by caseworkers, clients, and site supervisors as "workfare" jobs whose purpose was to allow clients to "work off their grants."

These intrinsic problems with basic education and CWEP assignments were exacerbated by inconsistent caseworker monitoring of clients' progress through these activities. The latter reflected caseworkers' lack of time to provide sustained attention to individual clients, the poor state of development of automated systems for tracking clients in assignments, and a lack of incentives for monitoring clients in JOBS placements. Monitoring tools had improved substantially by the end of the demonstration, when the new CRIS-E system was fully implemented. However, incentives for monitoring are unlikely to improve until the JOBS reward structure shifts from recognition of high rates of component participation to inducements based on evidence of successful activity completion and job placement.

Enforcing Participation

To enforce clients' obligation to work towards self-sufficiency, JOBS requires states to sanction those failing to cooperate with program requirements such as assessment and employment and training participation. Lurie and Hagen (1993) have observed that with inadequate funding to support universal participation, early state JOBS programs varied greatly in their approaches to sanctioning. Some states made little effort to enforce participation and ran essentially voluntary programs, while others attempted to enforce participation for a more narrowly defined segment of their mandatory caseloads.

Ohio's approach lies somewhere between these two extremes. Although the State never told CDHSs that the program goal was other than full participation, in practice resource constraints and weak monitoring of attendance in JOBS activities resulted in a relatively low

level of sanctioning.⁴ The fact that sanctions had to be implemented by income maintenance workers, rather than JOBS staff, also was mentioned repeatedly as an administrative hindrance causing workers to avoid their use.

Supportive Services

Ohio's state plan identifies a broad range of services provided to clients to facilitate JOBS participation and employment, including special counselling, child care, transportation, and a variety of other work expenses. However, actual utilization of these services likely was very modest during the early years of the State's JOBS program. Less than one percent of total Ohio JOBS expenditures in FY 1992 went to supportive services (excluding child care), far below the national average of 15 percent. Preliminary survey data suggest that child care concerns were an important factor discouraging clients from seeking work. Of 794 treatment-group members surveyed at the end of the demonstration, 35 percent reported that child care concerns kept them from looking for work during the preceding year.

5.4 SUMMARY

This concluding chapter has identified some of the program design and implementation factors that may explain why the impacts of Ohio's JOBS and Work Choice programs were modest. It is likely that a number of these problems already have been addressed in the time since the demonstration ended. Strengthening the State's JOBS program is essential to providing a sound foundation for the next generation of welfare reform in Ohio.

It would be a serious mistake to interpret the impact findings as evidence that Ohio's basic JOBS strategy is misdirected. Positive impacts were found for some clients and some JOBS activities. For these, and other subgroups and service strategies, the evaluation's capacity to measure results was limited by the slow pace of program implementation. Many clients did not receive their first program assignments for some time after they theoretically first became subject to JOBS requirements. Had the demonstration continued longer, the evaluation well

⁴ Among clients in the 1989 cohort, only 4 percent were sanctioned during the three-year follow-up period.

might have measured stronger impacts. It also is possible that impacts would be stronger now that unemployment has fallen.

The basic lack of education, job skills, and employment experience are crucial ingredients in the problem of welfare dependency. Although strengthening JOBS is unlikely to provide a sufficient answer to the problem in itself, it seems probable that it will be a necessary condition for broader reforms to succeed.

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